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LIGHTNING

By Prof. B. L. GOODLET, M.A., Member.

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SUMMARY

The first part of this paper deals with lightning as a physical phenomenon. The formation of thunderclouds is associated with convectional instability of the atmosphere, the separation of charge being effected by the resulting upward currents of air. Lightning strokes usually discharge negative electricity into the ground and consist of a preliminary "leader" stroke from cloud to ground followed by a powerful "return" stroke from ground to cloud. A complete flash usually consists of a sequence of such double strokes separated by time-intervals of the order of 10^{-2} sec. The distribution of flashes to earth under a storm centre is influenced by the distribution of space charge above the ground and by discontinuities of conductivity in the ground itself.

The second part of the paper deals with the effects of lightning strokes to overhead transmission lines. It is concluded that direct strokes to line conductors are certain to cause flashover: interruption to supply may be minimized either by devices to clear such faults (Petersen coils, expulsion protective gaps), or by overhead earth wires, which will prevent *any* strokes falling on the line conductors. Strokes to towers and earth wires are unlikely to cause flashover of a line insulated with 9 standard cap-and-pin units, if tower footing resistances are below 10 ohms; footing resistances low enough to prevent flashover with only 3 cap-and-pin units may be impossible to obtain.

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INTRODUCTION

This paper is an attempt to give a reasonably complete and consistent account of lightning phenomena. Although "Lightning" was the title of the Kelvin Lecture of 1929,* an account of this kind is opportune because more knowledge of lightning has been gained in the last seven years than during the previous seventy. As the paper is based almost entirely on other people's observations, the author must be permitted to remark that lightning is not a phenomenon which can be observed at will, and that anyone who attempted to write a comprehensive account of it without due regard for the experience of others would go sadly astray.

A study of recent electrotechnical literature might convey the impression that lightning is a blight peculiar to overhead transmission lines, on which it appears spontaneously as a travelling wave of form $e = E(\epsilon^{-at} - \epsilon^{-bt})$. To redress the balance, more than half the present paper is devoted to lightning as a physical phenomenon, and travelling waves are mentioned only when necessary.

(1) LIGHTNING AS A PHYSICAL PHENOMENON

(a) The Formation of Thunderclouds

A thunderstorm is a localized thermodynamic process in the atmosphere accompanied by electrical discharges.

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* *Journal I.E.E.*, 1929, vol. 67, p. 1269.

From the meteorological standpoint the latter are only a secondary effect of the main processes of condensation and precipitation. The cumulo-nimbus cloud or thundercloud differs from the ordinary rain cloud in having greater vertical dimensions, and in the existence of powerful upward air currents. As it is generally believed that the electrification of thunderclouds is associated with this upward air flow, it is necessary to consider how the latter can arise.

Any isolated mass of air having the same pressure but a higher temperature than the surrounding atmosphere (e.g. a fire balloon) will experience an upward acceleration of magnitude

$$a = g\frac{\theta' - \theta}{\theta}$$

where θ' = temperature of isolated mass, θ = temperature of atmosphere, and g = acceleration of gravity.

The pressure of the atmosphere decreases with height, and the heated air during its ascent will therefore expand, doing work against the atmosphere. The exchange of heat between the heated air and the atmosphere will be small in the absence of turbulent mixing —a condition excluded by the postulate of an isolated mass—and it may therefore be assumed that the expansion of the air is adiabatic; the work of expansion is then derived entirely from the internal heat energy of the expanding air, and the latter therefore undergoes dynamic cooling during its ascent.

The rate of decrease of temperature with height due to this cause can be readily calculated for dry air from the adiabatic equation*

$$\frac{d\theta'}{\theta'} = \frac{AR}{C_p} \cdot \frac{dp}{p}$$

together with the equation of the hydrostatic-pressure gradient, namely

$$\frac{dp}{dz} = -g\rho = -g\frac{p}{R\theta}$$

which give

$$\frac{d\theta'}{dz} = -g\frac{A}{C_p} \cdot \frac{\theta'}{\theta}$$

The quantity gA/C_p is termed the "dry adiabatic lapse rate" and has the value 9·72 deg. C. per km. Dry air ascending in an atmosphere of approximately equal temperature ($\theta' = \theta$) is therefore cooled at the rate of nearly 1 deg. C. per 100 metres of ascent.

Now the temperature of the atmosphere itself decreases with height at the average rate of 5–7 deg. C. per km. The net result is that *dry* air in adiabatic ascent cools at the approximate rate of 4 deg. C. per km *relative to the surrounding atmosphere.* For unaided ascent to be possible the isolated air must be considerably warmer than the atmosphere at the starting level, and the ascent per degree of initial temperature-difference is only some 250 metres. Convection under these conditions is therefore essentially stable.

Atmospheric air, however, normally contains an appreciable amount of water vapour. Humid air is specifically lighter than dry air and therefore tends to rise; its specific heat and therefore its adiabatic lapse rate below saturation are, however, almost the same as for dry air. If, however, damp air is brought to its dew point, further expansion results in the condensation of some of the water vapour and the release of a corresponding amount of latent heat. For a given expansion saturated air therefore cools much less than unsaturated air, i.e. the lapse rate of saturated air is smaller. This fact introduces the possibility of atmospheric instability. For, if the lapse rate of the ascending saturated air is less than the lapse rate of the atmosphere, the temperature of the ascending air relative to the surrounding atmosphere (i.e. $\theta' - \theta$) will increase with height and the ascending air will experience an increasing upward acceleration, until such time as the water vapour it contains is condensed and precipitated, and its lapse rate returns to the normal dry adiabatic value.

These considerations suggest* that the conditions necessary for the development of the strong upward air-flow characteristic of a thundercloud are: (α) High relative humidity of the lower atmosphere. (β) A steep lapse rate of temperature extending to a considerable height (e.g. 3 to 5 km.).

The principal ways in which these conditions arise are as follows:—

(i) *Heating of the lower air with a relatively cold upper atmosphere.*—This may come about either by the ground and lower air becoming heated by the sun, or by the base of a current of "polar" air becoming heated by travelling over the warmer sea. Most tropical storms and summer storms in the British Isles are of the first type; many winter storms, especially in Northern and Western Scotland, are of the second type.†

(ii) *The crossing of surface winds and colder upper-air winds.*—This often occurs in the southern quadrants of a depression where the surface winds are from more southerly regions than the winds of the upper air. Another likely place for such storms is the col between two anticyclones.

(iii) *The undercutting of warm air by the cold front of a depression.*

(b) The Electrification of Thunderclouds

The electrification of a thundercloud implies the separation of equal and opposite electric charges, and the separation of charges requires the performance of work. The only obvious source of this work is the upward air current through the cloud, and the question which most theories of thunderstorm electrification commence with is therefore: How can a separation of electric charges be effected by an upward air stream through a cloud? Opinions on the correct answer to this question differ considerably.

Sir George Simpson has suggested‡ that the required separation of charge is effected by the break-up of falling raindrops in the ascending air stream. It is a well-known experimental fact that the breaking-up of large water drops into smaller ones by an air current results in the water acquiring a positive charge while a negative charge is carried away by the air. The largest drop which can fall freely in air without breaking up has a diameter of about 0·5 cm. and falls with a terminal velocity of about 8 metres per sec. In other words, water cannot fall relative to the air with a velocity greater than 8 metres per sec., and consequently no rain can fall through an ascending air current the vertical velocity component of which exceeds this figure.

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* Here p, θ, and ρ are the pressure, temperature, and density respectively of the atmosphere at height z, θ' is the temperature of the isolated air at the same height, C_p is the specific heat at constant pressure (= 0·24), $A = 10^{-7}/4 \cdot 19$, and R is the gas constant.

* See Reference (1). † *Ibid.*, (2). ‡ *Ibid.*, (3).

Simpson assumes that immediately below the storm centre of a thundercloud the vertical component of wind velocity is greater than 8 metres per sec. Drops falling into this region from the upper parts of the clouds are therefore broken up into smaller droplets which acquire a positive charge; these droplets are carried up by the air stream, coalesce again, fall towards the storm centre, are again broken up, and so on. It is obvious that this process must result in a considerable accumulation of positively-charged water drops in the storm centre, while the upper portions of the cloud will acquire the negative charge carried away from the storm centre by the air stream.

This picture of the structure of a thundercloud leads to two important conclusions. First, the concentration of charge being greatest in the storm centre, the majority of lightning flashes may be expected to proceed from this region. Since the charge in this region is positive, lightning flashes should discharge positive electricity into the ground. Secondly, the negative charge of the thundercloud should lie above the positive charge of the storm centre.

Prof. C. T. R Wilson's picture* of the thundercloud is very different. Wilson measured the changes in the electric field at the ground during thunderstorms; his results indicated that the positive charge of the cloud was at a higher level than the negative charge. In other words, the base of the thundercloud is negative, a conclusion at variance with Simpson's theory. The mechanism suggested by Prof. Wilson to explain the formation of a cloud with a negative base is as follows: Suppose that the assumed charge distribution in the cloud already exists, producing a downward-directed field inside the cloud. In such a field positive ions move down, negative ions move up. The only ions which can exist inside a condensing cloud are, however, large ions with a mobility of about 0·0003 cm. per sec. per volt per cm. Direct experiment shows that the field intensity which will produce disruptive ionization in air containing water drops of the size encountered in clouds is of the order of 10 000 volts per cm. Greater fields than this cannot exist inside a cloud. The velocity of the large ions in the cloud will therefore not exceed some 3 cm. per sec. relative to the air. This is about the rate at which a water drop 0·1 mm. in diameter falls through air under gravity. All water drops with diameters greater than 0·1 mm. will therefore descend faster than the positive ions moved downwards by the field.

Now a water drop in a field of the direction assumed will have a negative charge induced on its upper surface and a positive charge induced on its under surface. The positive induced charge on the under-side of the drop will repel positive ions overtaken by the drop, but will attract the negative ions rising up to meet it. If the drop is falling faster than the positive ions, no positive ions can reach the drop from above, in spite of the induced negative charge on its upper surface. In this way the larger drops falling faster than the positive ions can collect an appreciable negative charge; the corresponding positive charge is carried to the higher portions of the cloud by the upward air stream. The fact that falling drops can acquire charges in this manner has recently been demonstrated by Mr. J. P. Gott.*

It is obvious that the mechanism outlined can only build up a field which already exists. Initially the only field penetrating the cloud is the normal fine-weather field of the earth; this is directed downwards, the upper air being positive with respect to the ground. The upper portion of the cloud should therefore build up positive with respect to the cloud base, which, according to Wilson's observations, is what actually happens.

One of the most outstanding features of Wilson's theory is that it also provides a mechanism for maintaining the positive charge of the upper atmosphere. The normal fine-weather vertical potential gradient at sea level is of the order of 100 volts per metre, corresponding to a surface density of negative charge on the earth of 3 electrostatic units per square metre. This field produces a conduction current between the air and the earth the average value of which is 2×10^{-6} ampere per km^2, or about 1 000 amperes for the whole earth. This current is sufficient to neutralize the entire negative charge of the earth in about 10 minutes and so abolish the field; yet this does not happen. It is therefore obvious that some compensating process must be at work to maintain the earth's negative charge.

Prof. Wilson points out that under a thundercloud with a negative base the direction of the vertical field is opposite to that usual in fine weather, so that the air earth current also flows in the opposite direction, as is required for the maintenance of the earth's negative charge. In the intense field under a thundercloud the normal current is greatly increased by currents due to point discharges from grass, trees, and other projections on the earth's surface. It has been shown by actual experiment† that the point discharge current from a single small tree under a thundercloud may exceed the current flowing to a square kilometre of ground in the fine-weather field. These point discharges under thunderclouds are often visible on high plateaux and on ships at sea, where they go under the name of St. Elmo's fire. Lightning discharges from a negative cloud-base to earth will also assist in maintaining the earth's negative charge. The complete circuit of the air-earth current is obtained by picturing the bipolar thundercloud between the ionized conductive layers of the upper atmosphere and the earth (see Fig. 1). Conduction to the upper atmosphere will occur by negative ions being drawn down from the upper atmosphere towards the upper positive pole of the cloud, thus leaving the upper atmosphere with the positive charge required to maintain the fine-weather field. This conception of the thundercloud as the source which maintains the normal fine-weather field is supported by the fact that the daily variations of the mean fine-weather potential gradient follow closely the daily variation of thunderstorm activity over the earth.[*]

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* See Reference (4).

* See Reference (5). A variation of this theory assumes that when a large drop overtakes and collides with a smaller one, the positive influence charge on the under-side of the large drop passes to the smaller one. The latter then rebounds, leaving the large drop with a net negative charge. The possibility of rebound without coalescence has been proved by experiment.

† *Ibid.*, (6).

The Simpson and the Wilson theories of the thundercloud are both exceedingly attractive, the former by virtue of its simplicity, the latter by virtue of its completeness. Although Wilson's theory started with a better experimental basis, the interpretation of field-change measurements is difficult and perhaps not entirely unambiguous.[†] In these circumstances it became very important to secure additional evidence to show whether the storm centre of a thundercloud is the seat of a positive or of a negative charge.

Simpson supported his picture of the thundercloud by a bold argument of analogy and a most convincing theory of spark propagation.[‡] Photographs of lightning flashes almost invariably show the discharge branching away from the cloud towards the ground. Discharges produced by a laboratory influence machine show such branching only in the direction away from the positive electrode. The presumption is therefore that branching of a discharge always occurs away from the seat of positive charge. This deduction, if correct, would establish the positive polarity of the storm centre. It is, moreover, difficult to conceive of a spark channel branching except in the direction of its propagation, and Simpson advanced a theory to show that a spark channel could elongate only in the direction away from the seat of positive charge.[§]

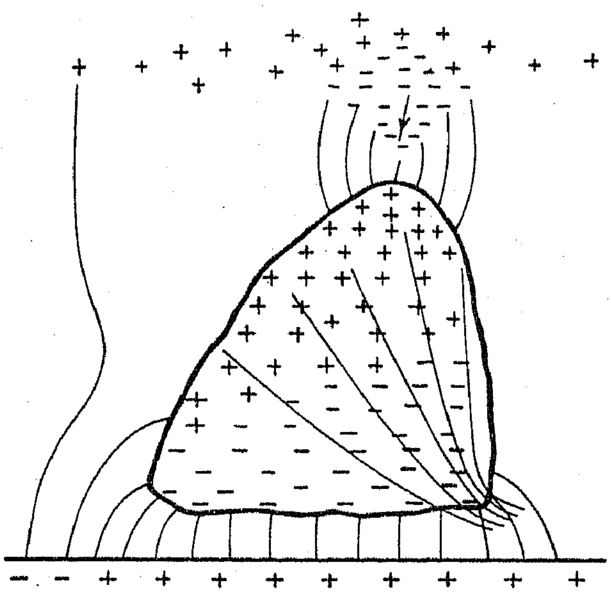

Fig. 1

Simpson apparently regarded these arguments as decisive, and his views, as outlined in the Kelvin Lecture[||] delivered to The Institution in 1929, obtained almost universal acceptance, at least in engineering circles.

The reversion from Simpson's views has come very recently. Additional evidence in favour of a negative cloud-base, from measurement of field-changes and wireless atmospherics, apparently convinced few people except the experimenters themselves. A serious blow to Simpson's theory was, however, given in 1931 by Allibone and Schonland,[¶] who showed that a spark channel branching *away* from the seat of negative charge could be produced experimentally in the laboratory. During the last 5 years a large number of measurements of the current carried by direct lightning strokes to transmission-line towers have been made with magnetic stroke-recorders. These measurements prove conclusively that in the vast majority of cases the cloud discharges negative electricity into the ground.

The present state of knowledge of the structure of the thundercloud may be summarized as follows. There is practically no doubt that the great majority of lightning strokes to earth proceed from a negatively charged region at the base of the cloud. There is, however, a considerable amount of evidence to show that regions of positive charge can also occur in the base of the cloud, much as postulated by Simpson, and that positive discharges towards the ground do occur.[*] Although Wilson's theory of the bipolar cloud fits many of the facts, it would seem that phenomena in actual thunderclouds are more complex than has been suggested by either Simpson or Wilson. Many observers believe that a thundercloud produces lightning only after the formation of a cap of false cirrus (which is a cloud form definitely associated with ice crystals) and that the formation of hail is an essential part of the process. Freezing temperature is usually reached at a height of 2–3 km., and above this level the cloud consists of ice crystals and water drops mixed together by the upward air stream. Friction between water and ice gives a negative charge to the former and a positive charge to the latter, so that a rather forced explanation of the positive charge of the cloud summit can be given. Dauzère[†] has proposed a theory the starting point of which is the positive electrification of the cirrus crystals in consequence of photo-electric emission caused by ultra-violet rays from the sun. Ross Gunn[‡] has suggested that the electricity of rain and thunderstorms can be explained by regarding each water drop and its surrounding ionized water vapour as an electrical concentration cell. The equilibrium charge on each drop is proportional to the radius, and is positive if the drop is evaporating and negative if the drop is condensing.[§] All these theories result in the cloud base acquiring a negative charge.

(c) The Mechanism of the Lightning Flash

Lightning flashes are initiated when the intensity of the electric field becomes greater than the air can withstand. Flashes may occur between the positively- and negatively-charged regions within the cloud, from the upper part of the cloud to the upper atmosphere,[||] and from the base of the cloud to earth. Discharges inside the cloud predominate in tropical thunderstorms, the altitude of which is in general greater owing to the

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[*] See Reference (7). [†] *Ibid.*, (8). [‡] *Ibid.*, (3).

[§] See Appendix I, where the fallacy in his argument is demonstrated.

[||] *Loc. cit.* [¶] See Reference (9).

[*] See Reference (10). [†] *Ibid.*, (11). [‡] *Ibid.*, (12).

[§] Electrification associated with the evaporation and condensation of water was observed by Lavoisier and Laplace in 1781, and was regarded by them as having a possible bearing on the electrification of thunderstorms. The matter came up again in 1840–42 in connection with Armstrong's hydro-electric machine. (See LAPLACE: "Complete Works," vol. 10, p. 203; *Philosophical Magazine*, 1840–42, vols. 17, 18, 20; M. FARADAY: "Experimental Researches," series 18, Nos. 2075–2145.)

[||] If Q_+ and Q_- are the positive and negative charges of a bipolar cloud, localized at points in the same vertical line at a distance d apart, then the field intensity at a distance h below the lower negative charge is

$$\frac{Q_-}{h^2} - \frac{Q_+}{(h+d)^2}$$

If Q_+ is diminished by a discharge to the upper atmosphere, the field below (Q_-) is increased and a flash to earth may be precipitated. This suggests that flashes to earth should be accompanied by upward flashes from the upper part of the cloud. Such flashes have in fact been observed [see Reference (13)].

greater height of the tropopause. In temperate regions clouds are lower and discharges to earth are therefore more common.

The suggestion that a lightning flash is only a very long electric spark was first advanced some 200 years ago. This view is substantiated by the electrical disturbances set up by the flash and by the identity of spark and lightning spectra. There are, however, important differences between a lightning flash and the comparatively short sparks produced in the laboratory. In the case of a spark between metal electrodes the charge is distributed over the surface of the electrodes and can flow easily towards the spark channel as soon as the latter is formed. In the case of the lightning flash, however, there are no electrodes. The charges producing the field are distributed not over a surface but through a volume. In a thundercloud the charge is carried entirely by water drops and large ions; small ions are practically non-existent except in regions of impact ionization. The conductivity of a cloud is consequently smaller than that of the air outside it, and a cloud can only be discharged by a number of ionized spark channels actually penetrating throughout its whole volume. To picture the thundercloud as a charged conductor is therefore entirely erroneous. It must never be forgotten that the visible lightning flash outside the cloud does not start or terminate at the cloud boundary but continues inside it in a maze of ramified and branching channels to which the water drops discharge by brushing. When the cloud is thin these channels can sometimes be observed.*

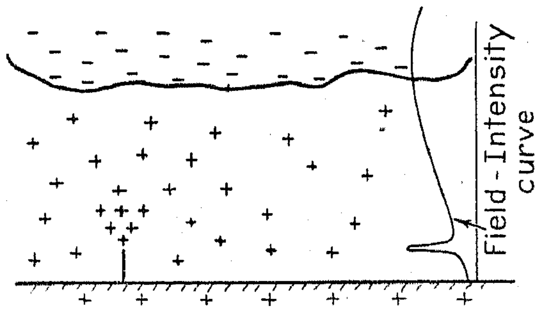

Fig. 2

The belief that the lightning flash originates in the clouds and strikes towards the ground is exceedingly ancient. Its basis is probably the fact that many flashes are seen which do not reach the ground or even travel towards it, so that it is natural to regard the cloud as the origin from which the flashes are propagated.

Laboratory experiments suggest that sparks are always initiated in the most intense portion of the electric field. The probable distribution of the field under a thundercloud is indicated in Fig. 2. The base of the cloud has a negative charge; the positive space charge in the air under the cloud is produced by point discharges from vegetation and projections from the earth's surface. The intensity of the field will be a maximum where the space charge changes sign, i.e. near the base of the cloud. The flash may therefore be expected to start near the base of the cloud and to develop away from this point in two directions: one branch will move upwards into the cloud, while another branch develops downwards towards the ground. A rough diagram of a developing lightning flash is shown in Fig. 3. Since electric current is a circuital quantity, the conduction current in the lightning channel must be continued as displacement current between the upper and lower branches, between the cloud branches and earth, and between the channel tip and earth. There will also be some conduction current in the ground. The positive space charge in the air under a thundercloud produces several important consequences. If, following C. T. R. Wilson and others, we assume a cloud with horizontal boundaries and a stratiform distribution of charge, so that the field varies only in the vertical direction, then the potential V and field ϵ at a height x above the ground plane are related to the volume density of the space charge by the equation

$$-\frac{d^2V}{dz^2} = 4\pi\rho = \frac{d\epsilon}{dz}$$

The intensity of the vertical field at height z is accordingly

$$\epsilon = 4\pi\int_0^z \rho dz + \epsilon_0$$

where ϵ_0 is the field intensity at ground level [i.e. $\epsilon_0/(4\pi)$ is the surface density of charge on the ground].

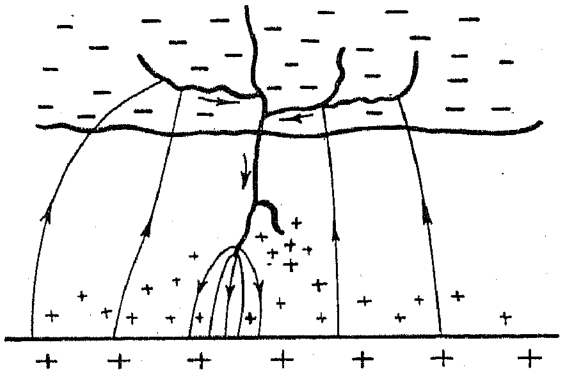

Fig. 3

The total space charge contained in a cylinder of unit cross-section extending up to the cloud of height h is therefore

$$\int_0^h \rho dz = \frac{\epsilon_h - \epsilon_0}{4\pi}$$

the total positive charge below the cloud (space charge + ground charge) being of course $\epsilon_h/(4\pi)$.

Now the field intensity at the cloud just before a flash cannot be much less than 10 000 volts per cm. (or 33 electrostatic units per cm.), while the field at the surface of the ground under the cloud is known to be quite small, measured values ranging between 50 and 500 volts per cm.*

It therefore follows that by far the greater part of the positive charge below a thundercloud is located not on the ground itself but as a space charge in the air close to the ground surface.† The assumed picture

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* See Reference (14).

* See Reference (15).

† If K is the mobility of positive ions, the current leaving unit area of ground is $i = \rho K\epsilon$. Writing $\rho = \frac{1}{4\pi}\cdot\frac{dF}{dz}$ and integrating, we get

$$\epsilon = \sqrt{\left[\frac{8\pi i}{K}z + \epsilon_0^2\right]}$$

$$\rho = \frac{i/K}{\sqrt{\left[\frac{8\pi i}{K}z + \epsilon_0^2\right]}}$$

The space-charge density ρ is therefore a maximum at the ground and falls off roughly inversely as the square root of the height.

of the charge distribution is of course much too simple, but the general truth of this conclusion is inescapable. A lightning flash, at least in its initial stages, is therefore a discharge between the negative space-charge region in the cloud and the positive space-charge region close to the ground. Only part of the negative charge given up by the cloud can reach the ground, the remaining portion being neutralized by the positive space charge. The effect of a flash being to neutralize some of the positive space charge close to the ground, it is not impossible for the ground field to be greater just after a flash than just before it. Increases in ground field following a flash have in fact been observed.

In considering the space-charge distribution it must be remembered* that the small ions produced by the point discharge are very liable to be captured by dust particles in the atmosphere. Their mobility in an electric field then becomes very small, and their motion, and hence the distribution of their space charge, will be determined mainly by the wind. In this way strong space charges may be built up at points where cloud form and ground contours produce favourable wind conditions. Such concentrations naturally tend to promote lightning flashes to these points.

The existence of the positive space charge under a thundercloud also explains why rain falling from the storm centre is usually positively charged—a fact which Simpson regarded as proof of the positive charge of the storm centre. The initial negative charge on a raindrop leaving the cloud base will be neutralized after it has fallen a very short distance through the positive space charge. After this the neutral drop, falling through a field directed upwards, will have a negative induced charge on its under surface and will collect a net positive charge by the Wilson mechanism already outlined.

Lightning flashes observed with the naked eye are often seen to flicker; this indicates that the light intensity, and therefore the current in the flash, are either pulsating or intermittent. The first definite evidence of the intermittent nature of lightning flashes was obtained in 1889 by L. Weber† and H. Hoffert,‡ who photographed lightning flashes in a moving camera and obtained a series of similar images spaced across the plate. This showed that the flash consisted of a number of separate strokes, following each other in *time* along practically the same path in *space*. Such flashes, now termed "multiple," were further studied by B. Walter§ and A. Larsen.|| The latter obtained a photograph showing 40 distinct discharges in a single flash, the total duration being 0·624 sec. and the intervals between successive strokes varying from 0·0026 to 0·0520 sec. C. V. Boys¶ devised a special camera with lenses revolving at very high speed, in the hope of being able to determine the direction and velocity of propagation of the flash from the sideways displacement of the images. He was unsuccessful for many years owing to the relative infrequency of lightning in this country. Working with a Boys camera in South Africa, B. F. J. Schonland** has, however, succeeded in obtaining photographs of 65 flashes consisting in all of 235 separate strokes. The conclusions reached by Schonland from the analysis of these photographs may be summarized in the following way.

Each flash is initiated by a streamer which shoots rapidly downwards from the cloud towards the ground, but becomes extinguished before it has travelled more than a fraction of the total distance to earth. Some 50 microseconds or so later a second streamer follows closely in the track of the first and elongates the channel a little further. A third streamer follows the second, and so the process continues in a series of "spits" until an ionized channel is blazed all the way from cloud to earth. The velocity of advance of each individual streamer may be as great as 1/10th the velocity of light but, owing to the pauses between streamers, the mean velocity of growth of the channel is much smaller—of the order of 10^7 cm. per sec. This series of streamers has been termed by Schonland the "stepped leader stroke." The stepped leader often branches in its advance towards the ground and this is the origin of the well-known branching of lightning flashes.

Immediately the stepped leader stroke reaches the earth the main or return stroke begins to travel up the channel from earth to cloud. Apparently the leader stroke draws from the cloud a very considerable charge which is distributed along the channel and branches formed by the leader. When this antenna system is connected to earth a discharge wave (the return stroke) at once travels up the channel. This view of the matter is suggested by the fact that the intensity of the return stroke (as shown by its luminosity) is greatest at the ground and decreases after each branch as it moves upwards. No case has been observed in which the intensity of the return stroke is greatest at the cloud. The velocity of propagation of the return stroke is of the order of 1/10th that of light. This leader–main stroke sequence has also been observed in the laboratory.*

After the extinction of the first main stroke there is a short pause of perhaps a few hundredths of a second, and then a second leader and main stroke occur. The leaders to the second and subsequent strokes of a flash usually travel from cloud to ground in a single flight. Schonland has termed these "dart leaders," to distinguish them from the stepped leader to the first stroke of the flash. The second and subsequent strokes are usually less intense and less heavily branched than the first stroke, but follow the same main channel very closely. The longer the interval between strokes the greater is usually the intensity of the subsequent stroke. The average number of strokes in a multiple flash is from 4 to 6, but a flash with 27 separate strokes has been observed with a total duration of about 0·5 sec. An idealized representation of the sequence of events in such a multiple flash is shown in Fig. 4.

The maximum current in the ground end of a lightning flash can be deduced from the intensity of the magnetic field produced in the vicinity of lightning conductors and transmission towers struck by lightning. A short bundle of iron wires† sealed up in a test tube is placed close to the tower leg with its axis along the magnetic-field lines. The residual magnetization of the bundle after a stroke is a measure of the magnetizing force to which it has been subjected. From the latter quantity the stroke current can be calculated. About 24 000 magnetic detectors of this kind were distributed over the lines of various German electric power companies in 1934. The results of the investigation to date[*] show that the most frequent value (mode) for the maximum stroke current is between 30 000 and 40 000 amperes. Currents greater than 100 000 amperes occur very seldom. In 97 per cent of the cases examined the cloud discharged negative electricity to earth, the cloud base being therefore presumably negative. Similar investigations have been made also in the U.S.A.[†]

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* See Reference (15).
† *Ibid.*, (16).
‡ *Ibid.*, (17).
§ *Ibid.*, (18).
|| *Ibid.*, (19).
¶ *Ibid.*, (20).
** *Ibid.*, (21).

* See Reference (22).
† Wires are used instead of a solid rod, to avoid time-lag due to eddy currents.

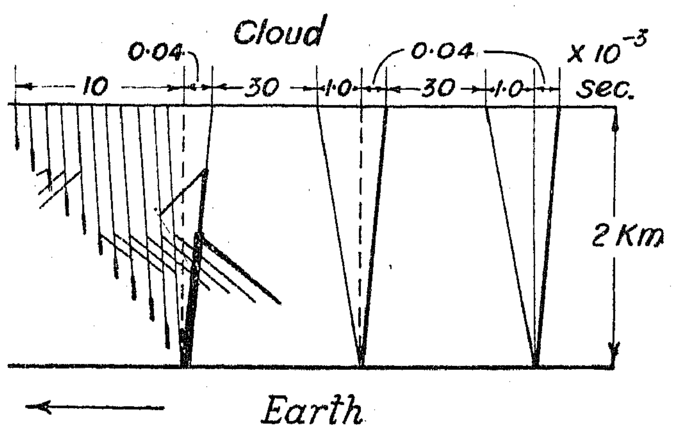

Fig. 4

The time variation of the current in a lightning stroke is more difficult to determine. As the leader stroke moves towards its goal, point discharge and displacement currents will flow, but the current in the object struck is probably small until the start of the main or return stroke. Counting time from this instant the current must rise to a maximum value and then decline again to zero, possibly with superimposed oscillations, as the stroke exhausts itself. We can therefore say *a priori* that the current/time characteristic of each stroke of a multiple flash must be similar to the curve in Fig. 5.

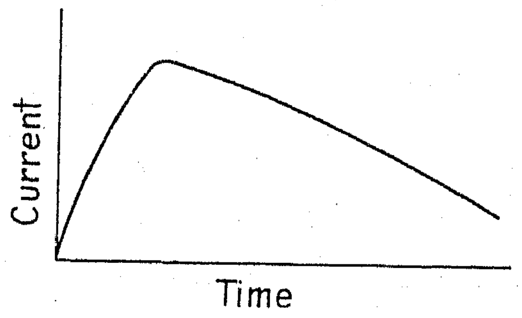

Fig. 5

The maximum rate of change of current in lightning flashes has been roughly measured[‡] by K. Berger,[§] whose results suggest that 20 000 amperes per microsecond is representative of the average stroke. Rates in excess of 40 000 amperes per microsecond have never been observed.

A rate of rise of 20 kA per microsecond in the most common peak-current range of 30–40 kA suggests a wave-front of at least $1\frac{1}{2}$ to 2 microseconds. The maximum rate of change observed (40 kA per microsecond) with a peak current of 100 kA, corresponds to a wave-front of $2\frac{1}{2}$ microseconds. Cathode-ray oscillograms of surges on transmission lines due to direct strokes usually show wave-fronts of 1 to 6 microseconds. Schonland[*] finds that the average time taken by the main stroke to travel from ground to cloud is about 40 microseconds, so that the total duration of the current pulse should be rather greater than this figure. It therefore seems probable that the typical lightning current-wave has a front of 2–6 microseconds and a total duration of 50 microseconds or more.[†] The charge carried to earth by such a current wave is of course given by the area $\int_0^\infty i dt$ under the curve. If the front and tail of the wave are assumed to be linear (triangular sawtooth waves) the charge carried will be $\frac{1}{2} \times$ (maximum current) $\times$ (total duration). With a duration of 50 microseconds and a maximum current of 100 000 amperes the charge conveyed is 2·5 coulombs per stroke. Schonland finds an average of 4 strokes per flash, the first being the most powerful, which suggests that the charge conveyed to earth by a powerful flash is not more than 10 coulombs. Prof. Wilson's estimate, from field-change measurements, is 20 coulombs or more. This discrepancy provides support for the view expressed above, that a large part of the charge which leaves the cloud goes to neutralize the positive space charge below the latter and never reaches the ground.

It is also interesting to examine the mechanism suggested by Schonland for the return stroke. Schonland[‡] says "The advancing leader with its branches constitutes an actual downward movement of part of the cloud charge, which is distributed over the whole conducting system and in particular over the branches at the moment the leader hits the ground. When this charged system is placed in good conducting connection with the earth a rapid flow of charge takes place on a scale and at a rate such as is shown by the main stroke of the discharge. The frequent concentration of the energy of the return stroke on the lower two-thirds of the channel as well as its decrease in velocity as it passes the branches, offers evidence that the leader process lowers into the air the greater portion of the cloud charge tapped."

The leader stroke thus forms and charges-up an "antenna" system, and the main stroke is the discharge of this antenna to earth. Now the stress ϵ at the surface of a long cylindrical conductor of length l, radius r, situated in free space and carrying a total charge Q, is roughly $\epsilon = 2Q/(rl)$. Assume that a total charge of 2·5 coulombs or $7 \cdot 5 \times 10^9$ electrostatic units (e.s.u.) is distributed over a total length of channel and branches equal to 5 km. or 5×10^5 cm. The dimensions of fulgurites, etc., suggest that the radius of a lightning stroke is not more than 10 cm. With this radius, however, the surface stress is about 3 000 e.s.u. per cm. or about 30 times the electric strength of air (30 kV per cm.). It is therefore evident that the charge lowered by the leader and conveyed to earth by the main stroke must be distributed throughout a considerably greater volume. The diameter of the cylinder 5 km. in length which will enclose 2·5 coulombs with a surface stress of 30 kV per cm., is some 6 metres. The mean volume density of charge is then $5 \cdot 3 \times 10^{-2}$ e.s.u. per cm^3. This value seems reasonable, since J. P. Gott* was able to produce artificial clouds with 10^5 particles per cm.^{3} each carrying 420 electron charges, corresponding to a volume density of 2×10^{-2} e.s.u. per cm^3.

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[*] See Reference (23). [†] *Ibid.*, (24).

[‡] The voltage induced in a loop of wire having a mutual inductance M to the leg of a transmission tower is (subject to various corrections) $M di/dt$, where i is the stroke current in the tower leg. Berger measured the maximum loop voltage, i.e. $(di/dt)_{max.}$, by means of a klydonograph.

[§] See Reference (25).

[*] See Reference (21).

[†] International Standards for impulse tests will probably be based on a wave with 1 microsecond front falling to half value in 50 microseconds.

[‡] See Reference (26).

An upper limit for the potential of the cloud base is obtained by assuming that the field intensity is 10 000 volts per cm. over the entire distance between cloud and ground; thus if the height of the cloud base is 1 km. its potential is then 10^9 volts.

(d) The Localization of Lightning Flashes in Certain Regions

A question often debated is whether or not certain identifiable localities receive an undue proportion of lightning strokes; in other words, does lightning show a preference for certain localities and if so what are the physical features which distinguish such localities? As much nonsense has been talked on this matter it will be discussed in some detail.

Consider a circular area, say, 10 miles in diameter. It is well known that the mean annual number of storms over such an area varies widely according to its position on the globe. Thunderstorms are frequent in the tropics but almost unknown in the polar regions. In a given latitude storms are more frequent on land than at sea, more frequent in mountains than over plains, and exceedingly rare over deserts. The reasons for this variation are also fairly well understood. The absence of storms in the polar regions is due to the dryness of the air and the impossibility of establishing the necessary vertical lapse rate of temperature. For equal insolation air over land becomes hotter than air over water, so that the lapse rate is greater; marsh land, farm land, and forest land all differ in their heat-absorbing power and evaporation rate. In mountain country, convection is assisted by the heating of the slopes (valley breeze) and the upward deflection of warm damp winds. Thunderstorms over deserts are rare because of the lack of moisture for cloud formation. The frequency of thunderstorms over an area is, however, only a rough index of the lightning hazard, since thunderstorms vary greatly in duration and severity. In particular, tropical storms, though frequent and severe, are higher up in the atmosphere (owing to the greater height of the tropopause near the equator) and the proportion of ground strokes seems to be smaller than for storms in temperate regions.

The phrase "localization of lightning flashes" is, however, usually given a more restricted meaning. It is widely believed that there are certain objects and points which are particularly liable to be struck by lightning whenever a storm is overhead. A belief of considerable antiquity,† for which there is much evidence, is that lightning strikes the highest objects in the vicinity. Sir James Frazer attributes the worship of the oak common to most Aryan races of Europe to the fact (?) that the oak is struck by lightning more frequently than any other tree and is therefore favoured by the thunder god. It is a common belief that outcrops of ironstone "attract" the lightning and that the remains of the yule log, if kept throughout the year, will prevent a house being struck. More scientific beliefs at present under discussion are: (i) That lightning "prefers" to strike soil of high conductivity, so that marshes, etc., are especially dangerous. (ii) That chimneys of ionized air which "attract" the lightning exist over certain kinds of ground rich in radioactive substances.

The evidence in favour of localization of lightning flashes comes mainly from the records of electricity undertakings. Thus Lehmann* found that in 9 years a certain 80-km. line received 43 strokes, of which 26 fell on one section 6·3 km. in length. Investigations showed that this section of the line was traversed by underground springs. It must be remarked that evidence of this kind is not conclusive unless it can be shown that the number and severity of the storms over the particular section are no greater than for the line as a whole. Moreover, electricity undertakings estimate the number of lightning strokes from the number of line flashovers; it is known that many strokes do not cause flashover† and that localization of flashovers can be caused by variations in the construction of the line itself. The responsible engineers also differ considerably in their ideas on the reason for such localization. Nevertheless, examination of all available data has left the author with the conviction that the existence of "danger spots," struck more frequently for a given storm exposure, is a fact and not an illusion.

The suggestion that geological factors influence the distribution of lightning strokes appears first in Arago's "*Notice Scientifique sur le Tonnerre*" (1838). Col. J. T. Bucknill, R.E., in 1881 made the statement "that lightning is most to be feared by those who live on well-conducting areas, even of low elevation, and least to be feared by those who live on non-conducting areas." The modern view is that the danger spots are located where there is a discontinuity in the geological formation. Thus Shipley‡ found danger spots associated with certain outcrops on the Nigerian plateau, while Lehmann§ found danger spots associated with underground springs. An interesting experiment bearing on this matter has been recently made by Stekolnikov.‖ A metal dish is filled with soil, a small hill is raised above the average level, and a metal ball is buried in the vicinity of this hill. When the dish forms one electrode of an impulse spark-gap, sparks pass to the hill when the soil conductivity is high but to the buried ball when the soil conductivity is low. In the opinion of the author the soil-conductivity hypothesis is well supported.

The air-ionization hypothesis has been the subject of an extensive investigation in France quite recently.¶ A Gerdien air-conductivity meter was mounted on a motor lorry, and measurements of air conductivity were made at a number of points over a large transmission system. The distribution of lightning strokes over this system was then examined for correlation with the distribution of air conductivity. No correlation was found with the total conductivity (i.e. total ion content) of the air, but it was concluded that lightning *did* tend to fall more frequently at points where the conductivity due to negative ions was greater than the conductivity due to positive ions.* The investigators identified high negative-ion conductivity with the presence of a negative space charge and pointed out that on Simpson's theory the lightning channel, being positive, would tend to be drawn to such regions.

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* See Reference (6).

† "Seest thou how God with his lightning smites always the bigger animals and will not suffer them to wax insolent, while those of a lesser bulk chafe him not? How likewise his bolts fall ever on the highest houses and the tallest trees?" (The *History* of Herodotus, Book 7, chap. 10.)

* See Reference (27).

† *Ibid.*, (23).

‡ *Ibid.*, (28).

§ *Ibid.*, (27).

‖ *Ibid.*, (29).

¶ *Ibid.*, (30).

The author's criticism of this work is twofold: First, the conductivity measurements were all made during fine weather; it is explicitly stated that during storms violent fluctuations occur and that either positive- or negative-ion conductivity may be in excess. This means that the values of conductivity and conductivity ratio measured in fine weather are no criterion of these quantities in stormy weather. But if air ionization has any influence on the path of the lightning stroke it must surely be the ionization actually existing at the time, not that measured, say, 3 months previously under different conditions. Moreover, ionization at ground level is not the same as ionization in the atmosphere.† Secondly, air conductivity is due principally to the small ions of high mobility, whereas space charge depends more on the large ions present. A greater negative-ion conductivity is not conclusive proof of a negative space charge. Since the lightning leader is almost certainly negative, Simpson's theory is beside the point.

What is the real truth on this whole question of the localization of lightning flashes? The author would suggest: First, that there are undoubtedly many localities where the configuration and nature of the ground favours the formation of a storm. Second, that the lightning flash is guided very considerably by the distribution of space charge of opposite sign below the cloud, produced by point discharge. Any object giving a copious point discharge, e.g. a mast or lightning conductor, is therefore particularly liable to be struck. Third, that lightning flashes tend to fall at places where there is a discontinuity of conductivity in the soil, e.g. on faults, outcrops, river banks, underground springs, buried pipes, etc.

(e) The Effects of a Lightning Flash

(i) Shattering of bad conductors.

The effects of a lightning flash show considerable diversity. A flash to earth produces a central hole rather like a molehill, with radiating furrows. In sand, tubes of fused glassy material termed fulgurites can be produced. Trees may be stripped of their bark, completely shattered, or set on fire. Heavy iron bands on a ship's mast have been broken when the mast was struck. Buildings are often set on fire, pieces of metal are melted, and many curious instances are reported of holes being knocked in stone or brick walls as by a projectile. Damage to trees and buildings is in general less when their surface is thoroughly wet. Fusing and firing are the natural result of a heavy-current arc, but how are the curious shattering effects to be explained? The author's opinion is as follows:—

The first point to note is that bursting and shattering occurs only when the objects struck are poor conductors of electricity such as wood, brick, or stone. Metallic objects when struck are either undamaged or simply melted. Many of the effects can in fact be imitated on a small scale in the laboratory.

If we pass a spark between two electrodes placed on the surface of a good dielectric such as glass, the spark will usually travel in the air adjacent to the surface. In the case of a poor dielectric such as wood, however, it may be easier for the spark to travel through the wood than through the air. This is almost always the case if the spark travels along the grain of the wood and the latter is not specially dried.

When the lightning leader stroke strikes the top of a tree it may therefore find it easier to reach the ground by burrowing under the bark through the moist sap wood than to continue along the surface of the tree, unless this surface is very wet. The damage done in this way might be small but for the main heavy-current stroke which travels up from earth along the path blazed by the leader. Laboratory experiments show that pressures of several hundred atmospheres may be developed if a heavy-current arc is confined within a tube with a bore smaller than the natural cross-section of the arc in free air. This is probably the explanation of all bursting and shattering effects in semi-conductors. The leader stroke blazes a tortuous path along the weak spots, joints, and crannies of the structure; the following main stroke blasts this confined path to more reasonable dimensions.

(ii) Damage to buildings.

Lightning damage to buildings* is a subject on which there is an extensive literature. The fundamental principle of protection is to provide means for the discharge to reach the ground without passing through a bad conductor such as wood, brick, stone, or concrete. This is the function of the lightning conductor.

It is of course by no means unknown for a lightning flash to miss the lightning conductor altogether. A more common occurrence is for a "side flash" to occur from the lightning conductor to a nearby metal roof gutter, or to a girder, water pipe, or gas pipe inside the building. Such side flashing is primarily due to the ohmic drop in the earth of the conductor, and to the surge impedance of the conductor itself; 10 ohms resistance in the earth and a current of 50 000 amperes in the conductor means that the potential of the conductor relative to its earthed surroundings is 500 000 volts. All heavy metalwork in a building should be solidly bonded. Lightning strokes falling on reinforced-concrete buildings having loosely-bonded reinforcing rods are liable to cause bad cracks at points where beams and floor slabs are joined to their supports. The cost of proper lightning protection for a building is very considerable, and it certainly would not pay to protect all buildings, because the annual upkeep and interest charges would be many times greater than the annual cost of lightning damage to the community. In many cases insurance is a cheaper proposition.

(iii) Oil-tank fires.

In April, 1926, lightning caused the destruction of 9 million barrels of oil storage in California. It is believed that this great fire was started by a small induced spark between two badly-bonded portions of a tank lid, which ignited explosive oil vapours. The best method of preventing such oil-tank fires† is to make the tanks gas-tight. A committee of the American Petroleum Institute found that lightning was the cause of 55 per cent of all oil-tank fires, but that gas-tight all-steel tanks were practically immune. This is borne out by experience with gasometers. Existing reservoirs with wooden lids can be protected by " divertor towers " about 150 to 200 ft. high to take direct strokes, together with a network of earth wires stretched about 10 ft. above the tank lid to prevent induced sparks.

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* See Reference (31).

† *Ibid.*, (32).

* See Reference (33).

† *Ibid.*, (34).

(iv) Damage to aircraft.

Some very interesting lightning problems occur in connection with aircraft.* Ten cases are known of British aeroplanes having been struck by lightning. In 9 out of the 10 cases the wireless aerial was hanging down and was damaged. In 6 of these cases the flash passed to the aerial through the fuselage from a wing tip or the propeller. Only one case is on record of a plane being struck with its aerial wound in. After an aeroplane has been struck its compass usually becomes deranged, owing to steel parts in its vicinity being magnetized. If these parts are movable, large variable errors will result. In several cases the engine of an aeroplane struck by lightning has become irregular; this is probably due to some damage in the ignition circuits or to demagnetization of the magnetos.

When an aeroplane or airship is flown through the intense electric field of a thundercloud the metal parts of the machine, unless bonded together, will acquire very different potentials, and shocks will be experienced by persons inside the aircraft when they touch and leave go of metal objects. A very graphic account of this phenomenon has been given by a German Zeppelin commander† who was compelled to fly through a thunderstorm on his way back from a raid over England during the War.

Moored kite balloons are particularly troubled by phenomena of this nature. A kite balloon is connected to the ground through a metal cable and is therefore at earth potential; the air in its vicinity is, however, at a considerable potential to earth. The fine-weather field is of the order of 100 volts per metre, so that the air at 1 000 metres will be at a potential of 100 000 volts to earth and therefore to the balloon. If storm clouds are near, this potential difference will be greatly increased and brush discharges will pass from the balloon to the air. When the balloon is wet these discharges may start from the fabric and set the balloon on fire. Manned kite balloons are therefore unsafe when there is any lightning in the vicinity.

(v) Effect of lightning on living creatures.

Direct strokes will not be considered here, since it is unlikely that any living creature can survive the close proximity of a 50 000-ampere arc. The less-direct effects are more interesting. The phenomenon known as St. Elmo's fire has already been mentioned. In the intense field at the top of a mountain this discharge is observed from metallic objects carried on the person, although the currents flowing are usually hardly perceptible. These currents are, however, greatly intensified in the vicinity of a lightning leader stroke moving towards the earth. The sudden increase in these point-discharge currents due to a nearby leader stroke is, in the author's opinion, the explanation of many curious shocks received from metal tools and domestic utensils when a flash strikes close by. This effect must not be confused with shocks due to the release of an induced charge on an object due to a flash perhaps half a mile away; induced shocks proceed only from objects of considerable electrostatic capacitance such as an unearthed wireless aerial or a surveyor's chain.

Earth currents can be a source of great danger. If a current of 50 000 amperes enters the soil at a point and spreads out uniformly in all directions the current density, and hence the voltage-drop, along the ground surface will be appreciable even at a considerable distance from the flash. The furrows which sometimes radiate from the point actually struck show that the voltage-drop may be sufficient to produce actual discharges through the soil. The voltage between two points on the earth separated by the length of an animal's stride may therefore be quite sufficient to pass an appreciable current up one leg and down the other. There are many cases of cattle-killing which can be explained in no other manner. When 126 sheep out of a flock of 152 are killed by a single flash it is hardly conceivable that they were all hit by the main channel.

Another danger to cattle arises from wire fences. A wire carried on the top of wooden posts 4 ft. high is fairly well insulated and, if struck, will probably rise to a high voltage for a considerable distance on each side of the stroke. Cattle leaning against such a fence may therefore be killed. In the same way telephone lines can introduce a dangerous voltage into dwellings.

There are several well-authenticated cases of lightning striking a pond or river and killing fishes therein. At first the author was considerably puzzled by this fact, since the current which could be passed through a fish in water seemed to be too small to be lethal. Apparently, however, the current required to kill a fish *is* fairly small, because the large South American electric eel which gives a shock of about 7 amperes at 15 volts kills all fishes within about a yard of itself. Lightning currents are of course much greater than this, even under water, so that perhaps the killing of fishes is not so surprising. It is also possible that the explosion wave radiating in the water from the point struck may have something to do with the matter. It is of course quite easy to produce luminous discharges in clean water by means of an impulsive voltage.

(f) Ball and Bead Lightning

Ball lightning is sometimes regarded as a subjective phenomenon. Although the accounts of many eyewitnesses are undoubtedly imaginative,* there is a mass of evidence to show that it really does occur. Recently Dr. Walther Brand of Marburg collected some 600 accounts of ball lightning, of which 215 were sufficiently detailed to inspire confidence in their accuracy. From these 215 accounts Dr. Brand† deduced the principal characteristics of the phenomenon. A translation of his summary is given below.

(i) Ball lightning (also termed a fireball or thunderbolt) is a ball-shaped (occasionally pear-shaped) electrical discharge of long duration which occurs occasionally during thunderstorms. Ball lightning occurs most frequently towards the end of a thunderstorm and is more frequent in winter. Its activity is less than that of ribbon lightning.

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* See Reference (35).

† *Ibid.*, (36).

* Some curious stories will be found in Camille Flammarion's " Thunder and Lightning " (Chatto and Windus, 1905).

† " Ball Lightning" (Hamburg, 1923).

(ii) Ball lightning generally appears as a red luminous ball of 10–20 cm. diameter surrounded by a blue contrast region and with a hazy outline. The balls may, however, be a blinding white and the outline is sometimes quite sharp.

(iii) A hissing, humming, or fluttering noise is usually heard.

(iv) On disappearing, fireballs often leave a sharp-smelling mist, which appears brown by transmitted light, blue by reflected light, and white when the air is saturated.

(v) The duration of the phenomenon varies from a fraction of a second to several minutes. The most usual duration is 3–5 sec.

(vi) A fireball may appear by descending out of the base of a cloud; it may also form as a floating ball, in free air or attached to some object. Very often ball lightning is preceded by an ordinary lightning flash, in which case the ball appears at or close to the point struck; this initial flash is, however, often absent.

(vii) Fireballs may disappear silently, with a mild crack, or with a blinding explosion—when a number of short streamers shoot out of the ball in all directions. Occasionally the ball is extinguished by an ordinary lightning flash striking it.

(viii) The velocity of a fireball which falls from the cloud base to the ground is very considerable (transition to ordinary lightning); close to the ground or in closed rooms fireballs usually move at about 2 metres per sec. A fireball may remain stationary for a time and an " attached " (*aufsitzende*) fireball can remain, boiling and emitting sparks, in its initial position until it disappears (transition to St. Elmo's fire). Sometimes a fireball appears to be moved by an air current, but in general its movement is independent of the wind.

(ix) Occasionally several fireballs appear round a place which has been struck by ordinary lightning. A single large fireball may burst into several smaller ones. Very occasionally two balls appear one above the other, bound together by a string of smaller balls, or such a string may appear attached to a single fireball [transition to true bead lightning (*perlschnur blitz*)].

(x) " Floating " (*freischwebende*) and " attached " (*aufsitzende*) fireballs appear to behave quite differently, although they can change into one another. Floating balls suggest a discharge of high voltage and small current; attached balls suggest a discharge of lower voltage but large current.

(xi) " Floating " fireballs have the red colour of meteorite tracks in the lower atmosphere. They shun good conductors and generally choose a path through the air. They are attracted towards closed spaces (i.e. houses) which they enter through the open window or door, sometimes even through small cracks; the chimney, with its conducting but non-inductive gases, is a favourite path, so that fireballs frequently appear in the kitchen from out of the fireplace. After circling the room several times the fireball leaves by some air path, often the one by which it entered. Floating ball lightning is not dangerous to human beings even when it appears in the middle of a group of persons; it appears to avoid them like it avoids good conductors. Occasionally a fireball makes two or three vertical oscillatory movements with an amplitude from a few centimetres to several metres. When these vertical oscillations are combined with a translatory motion the ball appears to progress in hops. Often the motion is confined to a single descent from the clouds to within a few metres above ground, followed by an immediate re-ascent.

(xii) " Attached " fireballs are of a blinding brilliance and white or blue in colour. They attach themselves to good conductors, preferring the highest points, or roll along such conductors (e.g. roof gutters). They heat the objects to which they are attached or along which they roll. They cause severe burns on the human body when they move over it (occasionally under the clothes) and produce lethal effects.

(xiii) The transformation of a " floating " into an "attached" fireball usually occurs by its making a dart to a good conductor in the vicinity. On touching the conductor it may either disappear, quietly or with an explosion, or it may continue as an " attached " fireball. Fireballs which fall from the clouds usually hit the ground and explode.

(xiv) The transformation of an " attached " into a " floating " fireball occurs by the ball simply rising from its support and floating upwards, usually along an inclined path, towards the clouds. In general such balls are extinguished very shortly afterwards.

No satisfactory theory of ball lightning has so far been developed. As the phenomenon cannot be observed at will it is likely to remain a mystery for some time to come. The most reasonable speculation is probably that of Prof. W. M. Thornton—that a fireball is a mass of ozone produced by the discharge, which suddenly reverts to oxygen with explosive violence.* Other theories are discussed in Brand's book and in a series of papers by E. Mathias.†

Bead lightning is a relatively well-known phenomenon. A recent letter published in *Nature*‡ states that in one case 20 to 30 beads about 3 inches in diameter and spaced about 2 ft. apart lasted about half a second after an intense flash. This account is typical.

(2) THE ELEMENTARY THEORY OF DIRECT STROKES TO TRANSMISSION CIRCUITS

(a) The Surge Impedance of the Lightning Channel

An estimate of the voltage set up on a transmission line by a direct lightning stroke can only be arrived at by making certain assumptions regarding the electrical characteristics of the stroke itself. A lightning stroke forms an ionized conducting channel extending upwards from the point struck. In one of the earliest papers on direct strokes Fortescue§ treated the lightning stroke as a conductor of 200 ohms surge impedance along which the lightning " wave " travelled from the cloud *down* to the line [Fig. 6(*a*)]. When a wave of amplitude E_0 moving along a line of surge impedance Z_0 arrives at a junction of surge impedance Z_j, the voltage E and current I produced at the junction are

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* See Reference (37).

† *Ibid.*, (38).

‡ *Nature*, 1936, vol. 137, p. 112.

§ See Reference (39).

$$E = \frac{2Z_j}{Z_0 + Z_j} E_0 = Z_j I \qquad \qquad (1)$$
$$I = \frac{2}{Z_0 + Z_j} E_0$$

On Fortescue's theory these equations give the voltage and current impressed on a surge impedance Z_j by a lightning stroke of potential E_0 and surge impedance Z_0.

If, however, we adopt Schonland's view, the leader channel which makes the initial contact with the system is already charged to the potential E_0, so that when contact occurs a discharge wave (the return stroke) flows *upwards* from the ground towards the cloud. The analogy which suggests itself is that of a cable of surge impedance Z_0 and charged to a voltage E_0 suddenly closed on to the surge impedance Z_j of the object struck [Fig. 6(*b*)]. The voltage E at the junction is then the difference of E_0 and the reflected wave e_r, while the current is the same in the cable and in Z_j. This gives

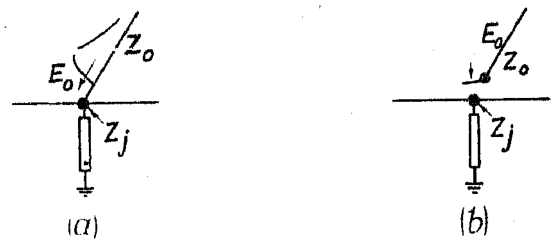

Fig. 6

$$E = E_0 - e_r$$
$$I = E/Z_j = e_r/Z_0$$

whence
$$E = \frac{Z_j}{Z_j + Z_0} E_0 = Z_j I \qquad \qquad (2)$$
$$I = \frac{E_0}{Z_j + Z_0}$$

For given impedances and a given current I entering the system the channel potential E_0 is twice as great on this second hypothesis. The simplest method of avoiding any confusion is to assume the value of the current I which enters the system from the stroke; the potential at the point struck is then $Z_j I$ on either hypothesis. The disadvantage of this method is that it is fairly certain that the stroke current is *not* independent of the impedance of the object on which the stroke falls.

An alternative method is as follows: Suppose that the stroke falls on a ground of zero resistance ($Z_j = 0$); the channel potential is then $\frac{1}{2}Z_0 I_g$ on the first hypothesis and $Z_0 I_g$ on the second. Inserting these values of E_0 in the corresponding voltage equations, we get, on either hypothesis,

$$E = \frac{Z_j Z_0}{Z_j + Z_0} I_g \qquad \qquad (3)$$
$$I = \frac{Z_0}{Z_j + Z_0} I_g$$

where I_g is the current which would flow if the stroke fell to a ground of zero resistance, and is thus a measure of the strength of the stroke. An estimate of the surge impedance Z_0 of the lightning channel can be obtained from the formula given in Appendix II for the electrostatic capacitance of a vertical wire (length l, diameter $2r$). In the case of a loss-free line the product of the inductance L and the capacitance C per unit length is equal to the square of the reciprocal of the velocity of light (c). Hence the surge impedance $\sqrt{(L/C)}$ is equal to $1/(cC)$. In this way we get

$$Z_0 = 30\left[2\left(\log\frac{2l}{r} - 1\right) - \log 3\right] \qquad (4)$$

Taking $l = 10^5$ cm., $r = 10$ cm., the surge impedance Z_0 comes out at 500 ohms. Estimates found in the literature vary from 200 to 600 ohms.

This method of treating the matter is of course open to serious criticism. A surge impedance calculated in this way is essentially the characteristic of a loss-free line along which waves are propagated with the speed of light. The lightning channel has in all probability quite a high resistance per unit length, and from Schonland's observations the velocity of propagation of the return stroke is considerably less than the velocity of light. While it is easier to criticize the method than to suggest a better one, it is well to recognize its uncertain basis.

The channel voltage E_0 which it is necessary to assume in order to account for observed discharge currents may be found by supposing the stroke to occur to a ground of negligible impedance. With a current of 100 000 amperes the channel voltage will lie between 10 and 60 million volts, according to the value selected for the surge impedance.

(b) Stroke to a Tower or Lightning Conductor

Suppose that the lightning channel terminates on a vertical conductor of height h and surge impedance Z_1 with an earth resistance R. The wave entering the top of the conductor [junction (0)] from the lightning channel at time $t = 0$ will have the amplitude

$$E = \frac{Z_1 Z_0}{Z_1 + Z_0} I_g \qquad (5)$$

This wave travels down the tower to earth [junction (1)], where it undergoes partial reflection. The reflected wave has the amplitude aE, where

$$a = \frac{R - Z_1}{R + Z_1} \qquad (6)$$

The wave aE travels upwards along the tower, reaching the top at the instant $t = 2h/v$, where v is the velocity of waves along the tower. At the top of the tower the wave aE undergoes partial reflection, the amplitude of the reflected wave being $\alpha a E$, where

$$\alpha = \frac{Z_0 - Z_1}{Z_0 + Z_1} \qquad (7)$$

The voltage at the top of the tower after the arrival of the first wave reflected from the ground is the sum of the original wave E and the refracted portion of the wave aE arriving from the ground. The latter is βaE, where

$$\beta = \frac{2Z_0}{Z_0 + Z_1} \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

The wave αaE travels down to the ground, is reflected as $a\alpha aE$, arrives at the tower top at time $t = 4h/v$, and produces there the voltage increment $(\beta a) \times (\alpha aE)$. Continuing the argument for any number of successive reflections, we get, for the voltage at the top of the tower, the value

$$e = \frac{Z_1Z_0}{Z_1+Z_0} I_g \left[1 + a\beta\left(1\left|\frac{2h}{v}\right| + \alpha a\left|\frac{4h}{v}\right| + \alpha^2a^2\left|\frac{6h}{v}\right| + \ldots\right) \right] \quad (9)$$

the terms $2h/v$, $4h/v$, etc., denoting that the corresponding waves only come into the calculation after the lapse of these times. The sum to infinity of the series with the round brackets is $1/(1 - \alpha a)$, so that the final voltage attained is

$$e_\infty = \frac{Z_1Z_0}{Z_1 + Z_0} I_g\left(1 - \frac{a\beta}{1 - \alpha a}\right) \quad . \quad . \quad (10)$$

On substituting from equations (5) to (8), it will be found that the final voltage is

$$e_\infty = \frac{R}{R + Z_0} Z_0 I_g = IR \quad . \quad . \quad . \quad (11)$$

This is simply the ohmic drop produced by the given lightning stroke I_g in an earth of resistance R. It is interesting to see after what lapse of time the reflections up and down the tower cease to be of importance.

Suppose that the tower is a steel cylinder 30 metres high and 2 metres in diameter, with a surge impedance [by equation (4)] of 150 ohms and an earth resistance of 10 ohms. With $Z_0 = 500$ ohms we have $a = -0{\cdot}875$, $\alpha = 0{\cdot}538$, $\beta = 1{\cdot}54$, $\alpha a = -0{\cdot}471$, $a\beta = -1{\cdot}35$. The time $2h/v$ taken by the wave to travel twice the height of the tower at a velocity of 3×10^8 metres per sec. is 0·2 microsecond. Hence

$$e = 115I_g(1{\cdot}0 - 1{\cdot}35|0{\cdot}2| + 0{\cdot}635|0{\cdot}4| - 0{\cdot}299|0{\cdot}6| + 0{\cdot}141|0{\cdot}8| - \ldots)$$

The values of e at various times after the start of the initial rectangular wave are given in Table 1.

The time variation of tower-top potential for 0/∞ and 1/50 microsecond waves is plotted in Fig. 7. It will be seen that the steady state $e = IR$ [equation (11)] is approached by a 2·5-million cycle oscillation which is damped out within 1·5 microseconds. At all times greater than this the surge impedance of the tower can be ignored, the tower potential being given by equation (11). If the stroke surge impedance is lower or the earth resistance higher than we have assumed, then the decay of the oscillation will be more rapid. With a sloping wave-front all voltage-changes are smoothed out and the tower surge impedance has even less influence. This justifies its entire neglect in the following approximate calculations of lightning voltages on transmission lines.

Table 1

Time, microseconds ..	0·1	0·3	0·5	0·7	0·9	1·1	1·3	1·5	1·7
Sum of terms in bracket	1·0	−0·35	+0·285	−0·014	+0·127	+0·061	+0·092	+0·078	+0·085
Tower-top voltage ratio $e/(IR)$	11·8	−4·11	+3·36	−0·165	+1·5	+0·72	+1·08	+0·92	+1·0

(c) Stroke to a Tower without Overhead Earth Wire

Suppose that a direct stroke of maximum current I occurs to the top of a tower supporting a 3-phase line without earth wire. One of the conductors of such a line is always at a positive potential to earth between $\frac{1}{2}$ and 1 times $\sqrt{2} \cdot \frac{E_L}{\sqrt{3}}$ (peak), where E_L = r.m.s. line voltage. The maximum negative potential attained by the tower is IR; the voltage across the insulators is the sum of the conductor and tower potentials, and sparkover will occur if this sum exceeds the impulse sparkover voltage V of the insulators. Thus sparkover *cannot* occur if

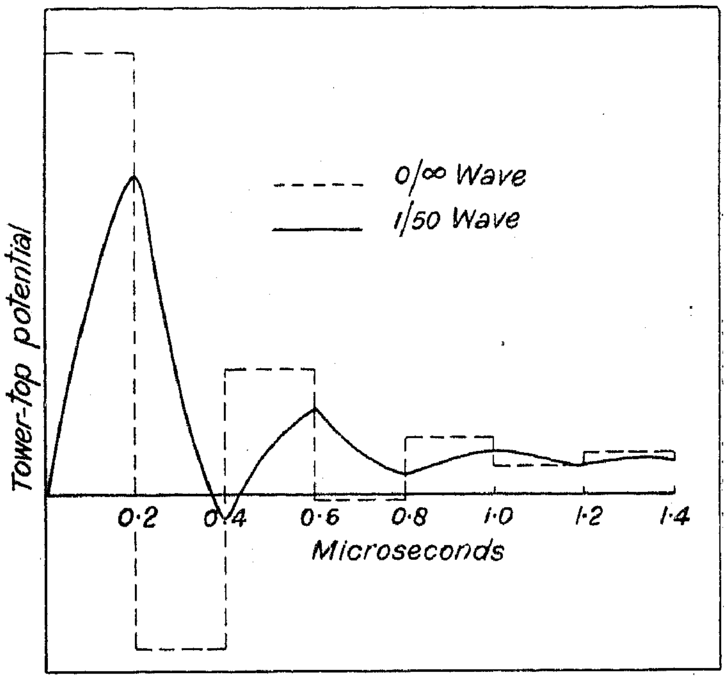

Fig. 7

$$IR < V - \sqrt{2}\frac{E_L}{\sqrt{3}} \quad . \quad . \quad . \quad . \quad (12)$$

and sparkover is *certain* to occur if

$$IR > V - \frac{1}{\sqrt{2}}\frac{E_L}{\sqrt{3}} \quad . \quad . \quad . \quad . \quad (13)$$

Suppose, for example, that the line operates at 132 kV and is insulated with 9 cap-and-pin type units of 5 in. pitch, with a gap of 40 in. between arcing horns. The minimum impulse sparkover voltage of such a string on a positive 1/50 microsecond wave is about 650 kV (peak).* Flashover will then not occur if $IR < 542$ kV, and is certain to occur if $IR > 596$ kV. The corresponding stroke currents for various values of footing resistance are given in Table 2.

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* See Reference (40).

This simple example shows the enormous importance of tower footing resistance in the case of strokes to a tower. Reducing the tower footing resistance on the above line from 10 to 5 ohms is almost as effective as doubling the line insulation.

Suppose now that the footing resistance is, say, 30 ohms. On a severe stroke sparkover will then occur from the tower to the most positive line wire when the instantaneous lightning current in the tower is between 18 and 20 kA.* The potential of the tower after this fault must be calculated on the assumption that half the self surge impedance Z_{11} of the faulted wire is in parallel with the footing resistance. The new equivalent footing resistance is therefore

$$R_{eq} = R\frac{Z_{11}}{Z_{11} + 2R} \quad . \quad . \quad . \quad . \quad (14)$$

If $Z_{11} = 500$ ohms the equivalent footing resistance will be 26·8 ohms, and the tower potential at the instant of sparkover will fall from (596/542) kV to (534/485) kV. This is the initial amplitude of the voltage wave e_1 propagated in both directions away from the fault along the faulted conductor (1). Now the voltage wave e_1 in the faulted conductor will induce voltage waves e_2, e_3 in the sound conductors. By equation (61) of Appendix III we have

$$\begin{aligned} e_1 &= Z_{11}i_1 \\ e_2 &= Z_{21}i_1 = e_1(Z_{21}/Z_{11}) \\ e_3 &= Z_{31}i_1 = e_1(Z_{31}/Z_{11}) \end{aligned} \quad . \quad . \quad . \quad . \quad (15)$$

Table 2

		5	10	20	30	50
Footing resistance R, ohms		5	10	20	30	50
Maximum stroke current I, kiloamperes	for no flashover	108	54	27	18	10·8
	for certain flashover	119	59	30	20	11·9

The wave voltages between wires are therefore

$$\begin{aligned} e_1 - e_2 &= e_1(1 - K_{21}) \\ e_1 - e_3 &= e_1(1 - K_{31}) \end{aligned} \quad . \quad . \quad . \quad . \quad (16)$$

where the coupling coefficients K are the values of the ratio of the corresponding mutual and self surge impedances or, what is practically the same, of the ratio of mutual and self coefficients of potential. On normal transmission lines the values of the coupling coefficients are usually between 0·1 and 0·3; 0·2 is probably a good average figure. The effect of coupling is thus to make the wave voltages between phases less than the voltage of the faulted phase to earth.

If the stroke current continues to increase after the first flashover, the tower potential will continue to rise. The instantaneous voltage between the tower and the next most positive wire at potential v is

$$R_{eq}I(1 - K) + v$$

and sparkover to this conductor will occur if this voltage exceeds the insulator sparkover voltage, i.e. if

$$R_{eq}I > \frac{V - v}{1 - K} \quad . \quad . \quad . \quad . \quad . \quad (17)$$

The potential v of the second conductor will be between $\pm \frac{1}{\sqrt{2}}\frac{E}{\sqrt{3}}$ according as the potential of the first conductor was half or full phase voltage to earth. The tower currents corresponding to these limits which will fault a second conductor are 26·8 and 32·8 kA. Sparkover to the third conductor may develop in the same manner.

(d) Stroke to a Line Conductor

If the stroke terminates on a conductor the total stroke current will divide, one half going off in each direction along the line. If I is the stroke current the voltage at the point struck is

$$E = \tfrac{1}{2}ZI = \frac{ZZ_0}{Z + 2Z_0}I_g \quad . \quad . \quad . \quad . \quad (18)$$

where Z is the self surge impedance of the faulted conductor. The space variation of the wave sent out along the line will correspond to the time variation of the voltage at the point struck. The voltage between the faulted and sound conductors will be reduced by coupling as given by equation (16). If, therefore, the line is struck close to a tower, so that the conductor potentials at the tower and at the point struck are practically identical, sparkover to the tower will occur if $E > V$, i.e. if

$$I > \frac{2V}{Z} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

or

$$I_g > \frac{Z + 2Z_0}{ZZ_0}V$$

If $V = 650$ kV and $Z_{11} = 500$ ohms, as before, any stroke current greater than 2 600 amperes or any earth current greater than 3 900 amperes will cause sparkover. Since practically all strokes carry greater currents, all strokes will cause sparkover. After sparkover to the tower has occurred matters will develop in the way discussed in the previous Section, it being immaterial whether the wire or the tower is struck first once the arc is formed.

Suppose now that the stroke falls at the centre of a span. The voltage gradient along the conductor is

$$\frac{\partial e}{\partial x} = \frac{1}{v}\frac{\partial e}{\partial t} = \frac{Z}{2v}\left(\frac{\partial i}{\partial t}\right) \quad . \quad . \quad . \quad . \quad (20)$$

If $Z = 500$ ohms, $v = 900$ ft. per microsecond, and Berger's figure of 20 kA per microsecond is accepted, the maximum possible gradient is 5·55 kV per ft. For a span of 900 ft. the potential at mid-span therefore cannot be more than 5·55 × 450 (or 2 500) kV above that at the tower. When the voltage across the insulators is 650 kV the potential at mid-span is 3 150 kV, about 80 per cent of which (or 2 520 kV) appears between faulted and sound conductors. The conductor spacing on a 132-kV line is about 12 ft.; the impulse breakdown voltage-gradient of long air-gaps is of the order of 200 to 300 kV per ft. It is therefore inconceivable that a stroke at the centre of the span should produce spark-over between conductors there, without also causing flashover of the insulators at adjacent towers.

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* Assuming, for simplicity, that the insulator sparkover voltage on the front of the wave is the same as on the tail.

(e) Stroke to a Tower with Overhead Earth Wire

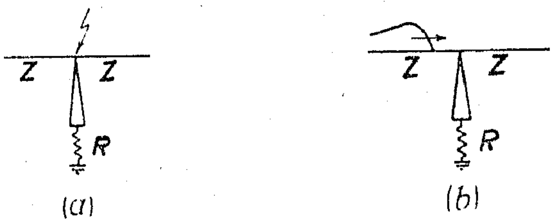
This case differs from (*c*) in that in calculating the tower potential the waves sent out along the earth wire and reflected back from adjacent towers must be considered. If the surge impedance of the tower is neglected the circuit diagram of the tower struck will be as shown in Fig. 8(*a*). The surge impedance of the junction for waves arriving from the lightning channel is then

$$Z_j = \left(\frac{1}{R} + \frac{2}{Z}\right)^{-1} = \frac{RZ}{Z + 2R} \quad . \quad . \quad . \quad (21)$$

The voltage initially impressed on the tower by a stroke of current I is

$$E = IR\frac{Z}{Z + 2R} = \frac{RZZ_0}{Z_0Z + R(Z + 2Z_0)}I_g \quad . \quad (22)$$

A wave of this amplitude is sent out in each direction along the earth wire and is reflected at adjacent towers, the reflected waves arriving back at the faulted tower after times $2S/v$, $4S/v$, $6S/v$, etc. (S = span, v = velocity). For waves arriving along the earth wire the impedance of the struck tower-junction is



(*a*) (*b*)

Fig. 8

$$Z'_j = \left(\frac{1}{R} + \frac{1}{Z} + \frac{1}{Z_0}\right)^{-1} = \frac{RZ_0Z}{Z_0Z + R(Z_0 + Z)} \quad (23)$$

The voltage produced by a unit wave at the junction being $2Z'_j/(Z + Z'_j)$, and since two equal reflected waves arrive simultaneously at the tower from opposite directions, the total voltage produced will be twice that of one wave. If, therefore, we use a refraction factor $\beta = 4Z'_j/(Z + Z'_j)$ we need only consider waves arriving from one direction. Thus

$$\beta = \frac{4RZ_0}{Z_0Z + R(2Z_0 + Z)} \quad . \quad . \quad . \quad (24)$$

The corresponding reflection factor $(\beta - 1)$ is

$$\alpha = \frac{2RZ_0 - Z(R + Z_0)}{Z_0Z + R(2Z_0 + Z)} \quad . \quad . \quad . \quad (25)$$

The circuit diagram of the adjacent towers is shown in Fig. 8(*b*). The refraction and reflection factors are respectively

$$b = \frac{2R}{Z + 2R} \quad . \quad . \quad . \quad . \quad . \quad . \quad (26)$$

$$a = \frac{-Z}{Z + 2R} \quad . \quad . \quad . \quad . \quad . \quad . \quad (27)$$

The potential of the faulted tower is best calculated by means of the reflection lattice diagram suggested by Bewley.* The faulted tower and three neighbours are shown in Fig. 9. The initial wave E travels away from the faulted tower O to the tower 1, where it is reflected as the wave aE and transmitted into the next span as the wave bE. The reflected wave arrives back at O after time $2S/v$, and produces there the voltage $a\beta E$. The course and effect of the other waves can be easily followed from the diagram. In this way the potential of the faulted tower is found to be

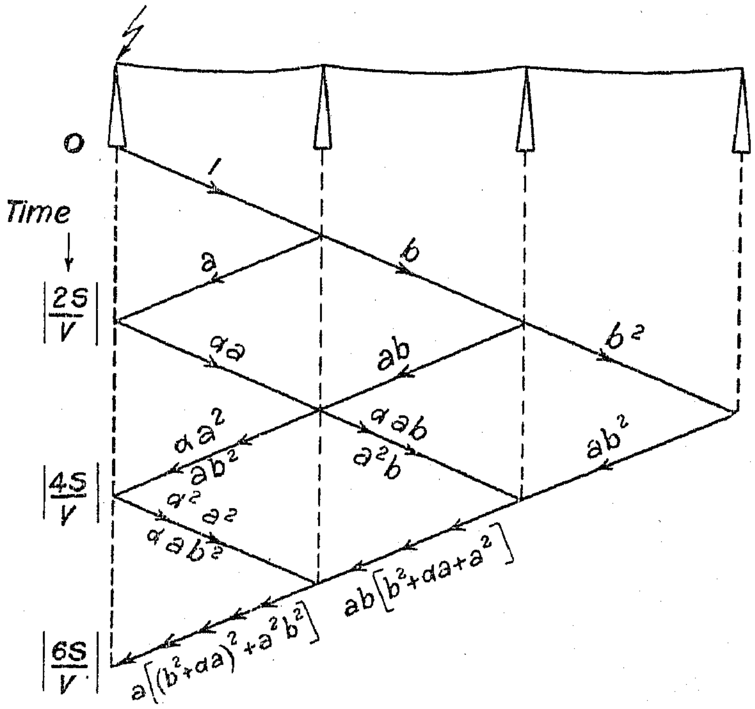


Fig. 9

$$e = IR\frac{Z}{Z + 2R}\left\{1 + a\beta\left|\frac{2S}{v}\right| + a\beta(b^2 + \alpha a)\left|\frac{4S}{v}\right| + a\beta\left[(b^2 + \alpha a)^2 + a^2b^2\right]\left|\frac{6S}{v}\right| + \ldots\right\} \quad . \quad (28)$$

Suppose, for example, that, as in Section (2)(*c*), $Z_0 = 500$ ohms, $Z = 500$ ohms, and $R = 30$ ohms. We then have

$$\beta = 0{\cdot}2036,\ \alpha = -0{\cdot}7964,\ b = 0{\cdot}1071,$$

and

$$a = -0{\cdot}893$$

Hence

$$a\beta = -0{\cdot}1815,\ (b^2 + \alpha a) = 0{\cdot}7225,$$
$$(b^2 + \alpha a)^2 + a^2b^2 = 0{\cdot}5306.$$

The factor $Z/(Z + 2R) = -a = 0{\cdot}893$. Hence

$$e = IR \times 0{\cdot}893\left(1 - 0{\cdot}1815\left|\frac{2S}{v}\right| - 0{\cdot}131\left|\frac{4S}{v}\right| - 0{\cdot}0964\left|\frac{6S}{v}\right| - \ldots\right) \quad . \quad (29)$$

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* See Reference (41).

IR is the potential of the tower without the earth wire, and the series therefore represents the reduction in tower potential due to its presence. Values of $e/(IR)$ at different times after the stroke are given in Table 3 for a span of 900 ft. and a wave velocity of 900 ft. per microsecond.

Table 3

Time, microseconds	1	3	5	7
Relative tower potential $e/(IR)$	0·893	0·73	0·614	0·528

We can now compare the performance of the assumed line with and without earth wire for direct strokes to a tower. The first effect of the earth wire is to reduce the peak tower potential by some 10 per cent (0·893 instead of unity). Secondly, the tail of the infinitely long lightning wave assumed is sloped, so that the tower potential falls to less than two-thirds of its initial peak value in 5 microseconds. The change in wave-form caused by the earth wire with a 1/50-microsecond stroke is shown in Fig. 10. This will increase the insulator sparkover voltage for a given peak potential by some 35 per cent.* Finally, the coupling between the earth wire and line wires will reduce the voltage between the latter and the tower. If the potential of tower and earth wire is e and the coupling factor between line and earth wires is K_g, the voltage between tower and line wire at the highest positive potential v is $[e(1 - K_g) + v]$, and sparkover will occur when this exceeds the sparkover voltage of the insulation. Assuming the latter to be the same as before (i.e. neglecting any effect due to the steeper wave-tail), the stroke current to produce sparkover is increased by the presence of the earth wire in the ratio

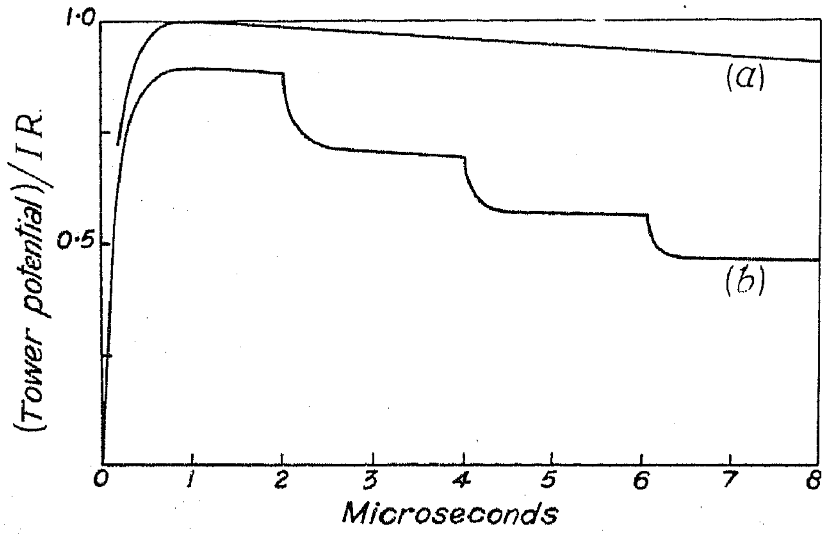

Fig. 10

(a) Without earth wire.
(b) With earth wire.

$$\frac{1}{1 - K_g} \cdot \frac{Z + 2R}{Z} \qquad (30)$$

In the given case this factor is 1·4, so that the maximum stroke current which will just not cause flashover is increased from 18 000 to 25 000 amperes. If we assume that the insulator sparkover voltage on the shorter wave is $1{\cdot}35 \times 650\,(= 880)$ kV, the current calculated from (12) and (30) is 36 000 amperes. The presence of the earth wire has thus doubled the lightning strength of the line, at least as regards insulator flashover. If the comparison is made on the basis of currents which the stroke would produce to an earth of zero resistance, the ratio of the currents which will not cause flashover with and without the earth wire is

$$\frac{1}{1 - K_g}\left[1 + \frac{2RZ_0}{Z(R + Z_0)}\right] \qquad (30a)$$

In the given case this ratio is almost the same, i.e. 1·39.

(f) Stroke to Earth Wire at Centre of Span

If a stroke hits the earth wire in the centre of the span the initial voltage produced will be $\frac{1}{2}ZI$, and a wave of this amplitude will travel away to each of the two adjacent towers and reach them after a time $S/(2v)$. This wave is transmitted into the next span with relative amplitude b [equation (26)] and reflected with relative amplitude a [equation (27)]; the latter wave arrives at mid-span after a time S/v and is reflected back to the tower with amplitude αa, where

$$\alpha = \frac{2Z_0 - Z}{2Z_0 + Z} \qquad (31)$$

The voltage at mid-span due to the arrival of the wave a is βa, where

$$\beta = \frac{4Z_0}{2Z_0 + Z} \qquad (32)$$

Continuing this argument up to the instant $3S/v$, at which the wave b transmitted past the adjacent tower and reflected from the tower $1\frac{1}{2}$ spans distant returns to the point of stroke, we get, for the mid-span voltage,

$$e_0 = \tfrac{1}{2}ZI\left[1 + a\beta\left(\left|\frac{S}{v}\right| + \alpha a\left|\frac{2S}{v}\right| + \alpha^2 a^2\left|\frac{3S}{v}\right|\right) + a\beta\,.\,b^2\left|\frac{3S}{v}\right|\right] \qquad (33)$$

while the voltage at the adjacent towers is

$$e_1 = \tfrac{1}{2}ZI\,.\,b\left(\left|\frac{S}{2v}\right| + \alpha a\left|\frac{3S}{2v}\right| + \alpha^2 a^2\left|\frac{5S}{2v}\right| + ab\left|\frac{5S}{2v}\right|\right) \qquad (34)$$

the last term in each case being due to the return of the wave from the second tower down the line. With $Z_0 = Z = 500$, $R = 30$, we get $\beta = 1{\cdot}33$, $\alpha = 0{\cdot}33$, $b = 0{\cdot}107$, $a = -0{\cdot}893$, $a\beta = -1.19$, $\alpha a = -0{\cdot}295$, and the tower potential is

$$e_1 = \tfrac{1}{2}ZI\,.\,b\left(\left|\frac{S}{2v}\right| - 0{\cdot}295\left|\frac{3S}{2v}\right| + (0{\cdot}087 - 0{\cdot}0955)\left|\frac{5S}{2v}\right|\right) \qquad (35)$$

The maximum potential attained by the tower is

$$e_{1max.} = \tfrac{1}{2}ZI\,.\,b = IR\frac{Z}{Z + 2R} \qquad (36)$$

Comparing (35) and (36) with (28), it will be apparent that the maximum tower potential and insulator voltage produced by a stroke at mid-span are the same as that produced by the same stroke current at the tower, but the duration of this potential and voltage in the former case is only half as great as in the latter. Values of $e_1/(IR)$ at different times after the stroke are given in Table 4 for a span of 900 ft. and a wave velocity of 900 ft. per microsecond.

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* See Reference (40).

Table 4

	1	2	3
Time, microseconds	1	2	3
Relative tower potential $e_1/(IR)$	0·893	0·63	0·554

On comparing Table 4 with Table 3 it will be seen that the decay of the tower voltage is considerably more rapid when the stroke occurs at mid-span. It is therefore safe to conclude that a stroke of current I which does not cause insulator flashover when it falls on the tower will not cause *insulator* flashover if it falls on the earth wire at mid-span. If the comparison is based on strokes of equal current I_g to earth, the insulator voltage is actually smaller—in the given case about 30 per cent less—when the stroke falls at the centre of the span.

With the same constants the potential of the earth wire at mid-span is

$$e_0 = \tfrac{1}{2}ZI\left(1 - 1{\cdot}19\left|\frac{S}{v}\right| + 0{\cdot}351\left|\frac{2S}{v}\right| - 0{\cdot}117\left|\frac{3S}{v}\right|\right) \quad . \quad (37)$$

Comparing (37) and (35), it will be apparent that the maximum voltage between earth and line wires at mid-span is greater than the maximum insulator voltage at the tower in the ratio

$$\frac{e_0}{e_{1max.}} = \frac{1}{b} = \frac{Z + 2R}{2R} \quad . \quad . \quad . \quad . \quad (38)$$

For the line considered, this ratio is 9·33. Values of mid-span voltage at different times are given in Table 5, while Fig. 11 shows the mid-span voltage for a 1/50 microsecond stroke.

Although the mid-span oscillation is heavily damped, its initial amplitude is large $(\frac{1}{2}ZI_0)$ for all wave-fronts with a duration T less than $|S/v|$, and is equal to $\frac{1}{T}\left(\frac{1}{2}ZI_0 \cdot \frac{S}{v}\right)$ for a wave-front $T > S/v$, assuming perfect reflection at the tower. In Section (2)(*e*) it was found that the line selected as an example would just withstand a 36 000-ampere stroke to a tower. If a stroke of equal current and a wave-front of 1 microsecond were to fall on the earth wire at mid-span the peak potential of the latter might reach $\frac{1}{2} \times 500 \times 36$ (= 9 000) kV, and the voltage between earth and line wires $0{\cdot}8 \times 9\,000$ (= 7 200) kV, assuming a coupling of 0·2. Although for voltage peaks lasting less than 1 microsecond the sparking gradient is at least 300 kV per ft., the clearance between earth and line wires required to prevent sparkover will probably not be less than about 20 ft. This rough calculation makes it quite clear that the distance between earth and line wires must be made considerably greater at mid-span than at the towers. The methods of obtaining this increased clearance are to string the earth wire with a smaller sag than the line wires, and to support the earth wires on vertical extensions of the main tower.

Table 5

		0·5	1·5	2·5	3·5
Time, microseconds		0·5	1·5	2·5	3·5
Mid-span voltage	$e_0/(\frac{1}{2}ZI)$	1·0	−0·19	+0·161	+0·044
	$e_0/(IR)$	9·33	−1·77	+1·5	+0·41

A considerable reduction of the mid-span voltage can be effected by using two earth wires, which also of course afford better shielding. For strokes to a tower these wires are in parallel, so that their surge impedance—and therefore the initial voltage peak—are reduced accordingly. To secure this condition for strokes falling in the span the two earth wires must be cross-bonded in the span. If suffixes 1 and 2 refer to the two earth wires and 3 to a line conductor, we have

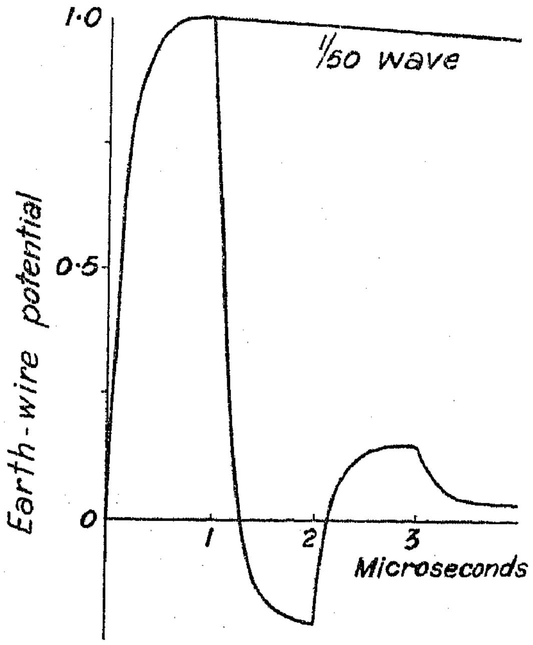

Fig. 11

$$\begin{aligned} e_1 &= Z_{11}i_1 + Z_{12}i_2 \\ e_2 &= Z_{21}i_1 + Z_{22}i_2 \\ e_3 &= Z_{31}i_1 + Z_{32}i_2 \end{aligned} \quad . \quad . \quad . \quad . \quad (39)$$

the Zi_3 terms being absent because the wave current i_3 is zero. With the earth wires in parallel $e_1 = e_2$, $i_1 = i_2$. The joint self surge impedance of the two earth wires is therefore

$$Z_{eq} = \tfrac{1}{2}(Z_{11} + Z_{12}) \quad . \quad . \quad . \quad . \quad (40)$$

while the coupling to the line wire is

$$K = \frac{e_3}{e_1} = \frac{Z_{31} + Z_{32}}{Z_{11} + Z_{12}} \quad . \quad . \quad . \quad (41)$$

Taking Z_{11} as 500 ohms and a 20 per cent coupling between individual wires (i.e. $Z_{12} = Z_{31} = Z_{32} = 100$ ohms), the equivalent surge impedance and coupling are 300 ohms and 0·33 respectively. The peak voltage between wires at mid-span for a 36 000-ampere stroke is therefore

$$\tfrac{1}{2} \times 300 \times 36(1 - 0{\cdot}33) = 3\,600\,\text{kV}$$

When the wave-front T is greater than the time S/v taken by the wave to travel the length of the span, the mid-span voltage is reduced in the ratio $\frac{1}{T}\left(\frac{S}{v}\right)$. The infrequency of mid-span flashovers suggests that stroke wave-fronts as short as 1 microsecond must be uncommon.

Table 6

Line without Overhead Earth Wire			
Insulator voltage for stroke to tower	$= RI$ (1 500)	or	$\frac{RZ_0}{R + Z_0} I_g$ (1 410)
Insulator voltage for stroke to line wire	$= \frac{1}{2}ZI$ (12 500)	or	$\frac{ZZ_0}{Z + 2Z_0} I_g$ (8 330)
Line with Overhead Earth Wire			
Insulator voltage for stroke to tower	$= (1 - K)\frac{Z}{Z + 2R} RI$ (1 070)	or	$(1 - K)\frac{RZZ_0}{Z_0 Z + R(Z + 2Z_0)} I_g$ (1 020)
Insulator voltage for stroke to earth wire at mid-span	$= (1 - K)\frac{Z}{Z + 2R} RI$ (1 070)	or	$(1 - K)\frac{2RZZ_0}{(Z + 2Z_0)(Z + 2R)} I_g$ (715)
Voltage between earth and line wires for stroke to earth wire at mid-span, or for stroke to line wire	$= (1 - K)\frac{1}{2}ZI$ (10 000)	or	$(1 - K)\frac{ZZ_0}{Z + 2Z_0} I_g$ (6 660)

(g) Conclusions

To facilitate comparisons the formulae for the peak voltages set up in the various cases are given in Table 6, in terms both of the actual stroke current (I) and the current (I_g) which the same stroke would deliver to a zero-resistance earth.

The figures in brackets are the calculated values of these quantities in kilovolts for $Z = Z_0 = 500$ ohms, $R = 30$ ohms, $K = 0{\cdot}2$, and I or $I_g = 50\,000$ amperes. The alternating voltage on the conductors has been neglected. The conclusions which may be drawn from the foregoing analysis are as follows:—

(i) The insulator voltages set up by strokes to a line conductor (about 10^7 volts) are of a different order of magnitude from those caused by strokes to the towers or earth wire (about 10^6 volts). It is impracticable to insulate a line to withstand direct strokes to the line conductors without flashover.

(ii) An overhead earth wire which intercepts *all* direct strokes will reduce insulator voltages to a manageable order of magnitude, provided that the tower footing resistance is sufficiently low. A line of 900 ft. span insulated with 9 cap-and-pin units should be almost immune from lightning if all footing resistances are below 10 ohms.

(iii) When an earth wire is present, insulators are stressed most highly by strokes to the towers, while line clearances are more highly stressed by strokes to the earth wire in mid-span.

(iv) The peak voltage between earth wire and line wires for a stroke in mid-span is less than its theoretical maximum if the wave-front is greater than the span length. The mid-span voltage can be reduced by the use of two earth wires, bonded in the span.

(v) The wave-form of the impulse voltage applied to the insulators prior to flashover is similar to the applied lightning wave only on lines without earth wires. If the stroke occurs to an earth wire the tail of the applied wave is considerably shortened, a 1/50 microsecond wave being converted into something like a 1/5 microsecond wave. This results in an increased insulator sparkover voltage. After a flashover, all wave-forms are very distorted.

The preceding calculations are intended to show, in a general way only, what factors determine the voltage produced in a transmission line by a direct stroke. The conclusions reached must be accepted with caution, since the numerical values assumed for the various constants are probably in error and various secondary phenomena have been neglected to simplify the argument. It is worth while considering briefly where assumptions of doubtful validity have been made.

First, the value assumed for the surge impedance of the lightning channel—namely 500 ohms—is almost a pure guess, and indeed it may well be that the lightning channel cannot be regarded as possessing a surge impedance at all. The author will therefore not quarrel with a reader who feels that 200, 400, or 600 ohms is a better value.

Secondly, the value assumed for the surge impedance of a line conductor—namely 500 ohms—is almost certainly too high, while the value assumed for the coupling between conductors (0·2) may well be too low. It is known that at very high voltages the measured surge impedance of a conductor decreases, perhaps even to 300 ohms, owing to the increase in the effective diameter of the conductor caused by corona; the coupling between conductors may be considerably increased owing to the same cause. This effect has been ignored because its magnitude is difficult to estimate in the absence of experimental data, and because it tends to diminish the voltages produced. The calculations given thus apply to the worst possible condition, in accordance with orthodox engineering tradition.

Thirdly, the simple theory of travelling waves with only a single velocity of propagation has been used throughout. With high voltages on the conductors and high-resistivity soil, waves of several different velocities must occur. The results of the multi-velocity theory are not very different from those of the simple theory for distances of only a few spans, and the voltages set up close to the point struck are therefore almost the same on either theory. According to the simple theory, however, insulated conductors not involved by the flash simply float in the electrostatic field and no free travelling waves are induced on them. The multi-velocity theory shows that such free travelling waves *can* be induced on sound conductors, so that the voltages applied to the terminal equipment of the line will be different on the two theories. This matter cannot be pursued here.

Readers who wish to make similar calculations for their own systems should remember two important facts. First, the impulse sparkover voltage of a given insulator or air-gap is not constant but varies according to the shape of the impulse wave applied. Thus for the 9-unit chain with 40-in. horn spacing assumed in the examples, the minimum impulse sparkover with tower negative is 650 kV for a 1/50-microsecond wave but nearly 900 kV for a 1/5-microsecond impulse wave. For a stroke to a line wire the line will be negative, and slightly different values will apply. If sparkover occurs on the front of a wave of high amplitude, still other values, varying with the rate of voltage-rise, must be employed. Secondly, the resistance of an earth passing a large impulse current may be very different from the resistance of the same earth measured with a portable earth-tester. For consider an earth connection consisting of, say, a 2-in. diameter tube with a buried length of 5 ft. The contact surface of the tube is 120π sq. in., or 2 430 cm^2. The mean current density for a current of 10 000 amperes is therefore 4·1 amperes per cm^2. The voltage gradient at the tube surface in soil of 10 000 ohms/cm. cube resistivity is therefore $4\cdot 1 \times 10$ (or 41) kV per cm. It is hard to believe that soil will support an electric field of this magnitude without some kind of disruptive discharge, which will lower the effective resistance of the earth. It may therefore be expected that the resistance of an earth passing a large impulse current will be *less* than its test value, and such experiments as have been made fully confirm this view. If the area of the earth electrode is made large in order to secure a low resistance, the current density and therefore the decrease in resistance with a heavy current may not be large; nevertheless, lightning voltages calculated on the basis of test values of earth resistance are almost certain to be on the high side.

(h) General Discussion

It is believed that rather more than half the direct strokes to a line fall on the conductors in the span. The final potential of an insulated conductor so struck is probably never less than 2 million volts, and may be 10 times as great. The insulator impulse sparkover voltage of the most heavily insulated line yet constructed is of the order of 2½ million volts. Wooden poles give additional insulation to earth but cannot be relied on to withstand more than 100 to 150 kV per ft.; even if such poles are not shattered it is extremely probable that a side flash will occur along the cross-arm to a neighbouring conductor, thus involving more than one phase. The conclusion that practically every direct stroke to a live wire will produce some kind of flashover, irrespective of the line insulation, therefore seems inescapable. The only two possible methods of preventing line outages are therefore: (i) To prevent direct strokes to line conductors. (ii) To render line flashovers innocuous.

The prevention of direct strokes to the line conductors is the function of the overhead earth wire. Single earth wires (as employed on the grid lines) are believed to be reasonably effective interceptors, but in order to ensure protection in severe lightning areas two earth wires, bonded at mid-span and mounted some 15 to 20 ft. or more above the line conductors, are necessary. This construction has been adopted on the 287-kV Boulder Dam line* and on several 220-kV lines in Russia and elsewhere. It must, however, be emphasized that the efficacy of earth-wire protection depends upon the footing resistances being so low that the tower potential due to the lightning current is less than the flashover voltage of the insulators. The lower the insulation level of the line the lower must be the footing resistance; since the latter cannot be reduced indefinitely, it is evident that earth-wire protection will not be effective below a certain level of line insulation.† If footing resistances are nearly but not quite low enough, a slight increase in the insulation level of the line may improve its outage record.

In order to secure low footing impedance in dry soil many modern lines are equipped with a counterpoise. This consists of a length of bare cable buried in the ground and attached to the foot of the tower. Counterpoises are termed "parallel" or "perpendicular" according to their direction relative to the line. The complete theory of the counterpoise is complex‡ and many points are still uncertain; the main principles are as follows. The surge impedance of a bare cable buried in the ground, defined as the ratio of instantaneous voltage and current obtained from cathode-ray oscillograms, is initially of the order of 100 to 200 ohms; after a time, which is of the order of 2 microseconds for a 200-ft. length, the surge impedance falls to a much lower value, governed by the resistivity of the soil. A perpendicular counterpoise can therefore be treated as a surge impedance in parallel with the tower footing resistance, a parallel counterpoise as magnetic coupling to the line and earth wires, which makes its beneficial effect slightly greater. Each circuit of the new Boulder Dam line is equipped with two parallel counterpoises, continuous from tower to tower.[*]

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* See Reference (43).

† If $R = 5$ ohms as a minimum and $I = 50\,000$ amperes, then the tower potential (with 20 per cent coupling) will be 200 kV and the maximum insulator voltage somewhat greater (221 kV on a 44-kV system). The equivalent of 3 suspension insulators will therefore be required to prevent flashover.

‡ See Reference (44).

A most difficult earthing problem arises when transmission towers have to be founded on rock, as frequently happens in mountain districts. In such cases it may be impossible to obtain low footing resistance even with a counterpoise (which is difficult to lay under these conditions), and there is certain to be a heavy discharge round the base of the tower when the latter is struck; the author has heard of such a rock foundation being cracked by lightning.

A recent suggestion is that minor transmission lines should be protected by means of lightning diverters, i.e. vertical lightning rods of self-supporting design erected at intervals along the line. If h_1 is the height of the diverter and h_2 that of the live wires, the recommended horizontal interval between diverters is $8(h_1 - h_2)$, the diverters being spaced at least 5 metres from the trace of the line on the ground. A variant of the same idea is to fit ordinary lightning conductors to each line pole, projecting some 15 ft. above the top of the pole. Lines protected by diverters and lightning rods are in service in Russia, but with what success is at present unknown.

The alternative to lightning-proof construction of the line is to ensure that lightning flashovers on the line do not interrupt the supply. The best-known method of doing this is of course the Petersen coil, which is in general use on the Continent but is less popular in this country and the United States. Its merits are, however, recognized in a recent American report[†] which states that, on a 250-mile system operating at 140 kV with Petersen-coil protection, over 90 per cent of line-to-earth faults and 70 per cent of all faults were cleared without interruption of supply. The other method of suppressing line flashovers is the expulsion protective gap.[‡] This consists of a thick-walled fibre tube fitted with a plug electrode at one end and a ring electrode at the other; the axial distance between these electrodes is much less inside than along the outside of the tube, so that flashover always occurs internally. This tube is mounted like an arcing horn on the tower, the ring electrode at the open end of the tube being the one usually earthed to the tower. When a sufficiently high voltage appears on the line, flashover occurs from the line to the plug electrode and thence inside the expulsion tube to earth. The arc inside the tube liberates gas from the tube walls which, together with blast action, promotes rapid deionization at the current zero. Expulsion gaps operate satisfactorily only between certain current limits, but within these limits are capable of rupturing a heavy arc after only one or two half-cycles. Although the weathering qualities of the fibre tube are not very good, expulsion protective gaps seem to have proved satisfactory in actual service.[*]

If the author may venture a prediction it is that the second solution of the lightning problem will prove the more popular. Whereas the cost of constructing a really lightning-proof line with adequate earth wires is very considerable, arc-rupturing devices and lightning arresters can be installed on existing unsatisfactory lines.

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[*] See Reference (43).

[†] *Ibid.*, (45).

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APPENDIX I

The Propagation of Long Sparks and Lightning Flashes

General.

Observations on long sparks* and lightning flashes† have established beyond reasonable doubt that the ionized spark channel is initiated at the point of maximum intensity of the field and subsequently elongates, in the general direction of the field lines, until it bridges between the surface or volume charges producing the field. On this view, the time of breakdown or time-lag of a spark-gap is simply the time taken by the spark channel to travel the required distance. An elongation of the spark channel is synonymous with the ionization of a corresponding volume of air, and it is therefore obvious that propagation can continue only so long as the field intensity at the advancing tip of the channel remains high enough to produce the required ionization. If a small increase δx in the length of the channel results in a decrease of the intensity ϵ at its tip (i.e. if $\delta\epsilon/\delta x$ is negative) then propagation will very soon cease.

A good idea of the conditions governing propagation is obtained by picturing the spark channel as a straight conducting wire advancing in the direction of its axis towards a large plane electrode (point-plane gap). If the voltage applied between wire and plane is constant, the intensity at the tip of the wire will increase as the wire advances. The tip intensity can, however, be made to decrease, in spite of the approach of the tip to the plane, by reducing the voltage at a sufficiently rapid rate during the advance. This is the principle of the "suppressed discharges" of J. J. Torok,‡ produced by arranging for the applied voltage to collapse to zero before the spark channel completely bridges between the electrodes. The procedure adopted by Torok was to apply a high impulse voltage to two spark-gaps connected in parallel, one of which—the so-called "chopping" gap—being set slightly closer than the gap under test. Both gaps commence to break down together, but the process is completed first in the "chopping" gap with the closer setting; the voltage on the test gap therefore collapses before the streamers formed in it have time to bridge between the electrodes. By adjusting the setting of the chopping gap it is thus possible to arrest the streamers in the test gap at any stage of their development.

It seems also possible for suppression of a streamer to be brought about by excessive voltage-drop along its length. If C is the capacitance of the equivalent wire per unit length, v its velocity of advance, and V the constant voltage between its tip and the plane, the charge which has to be supplied during an elongation dx is $CVdx$, corresponding to a current CVv. Thus if $C = 10^{-12}$ farad, $V = 10^6$ volts, and $v = 10^7$ cm. per sec., the charging current of the advancing wire will be 10 amperes. If the wire has a high resistance per unit length the voltage between tip and plane (which governs the tip intensity) will be less than the total applied voltage by the amount of the resistance drop due to the current in the wire. This suggests that the conductivity of the spark channel at any instant must have an important influence on its propagation.

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* See Reference (22). † *Ibid.*, (21). ‡ *Ibid.*, (48).

The best-known theory of the propagation of a spark channel is that of Simpson,[*] based on the relative immobility of the positive carriers (ions) as compared with the negative carriers (electrons) produced by impact ionization. If a short length of conducting spark channel is situated like a needle in a field of force, influence charges will be formed at its ends. When the tip intensities are sufficient to cause impact ionization the negative carriers produced move towards the positive end of the channel, leaving behind a residue of immobile positive ions which in effect extend the spark channel; at the negative end, however, the immobile positive ions cannot move towards the tip while the negative carriers move away from it, forming a diffuse cloud and so weakening the negative tip-intensity. This mechanism obviously allows propagation of the channel to occur only in the direction of the field, i.e. from a positive towards a negative charge. Simpson, in fact, denied the possibility of a spark channel growing in the opposite direction and considered that as branching of a channel must occur in the direction of its propagation, i.e. away from the seat of positive electricity, such branching enabled the positive end of a spark or a lightning flash to be identified. Since it is an experimental fact[†] that both propagation and branching can occur away from the seat of negative charge, it is obvious that Simpson's theory in its original form is untenable. The following theory is believed to be original.[‡]

Theory of Spark Propagation.

Referring to Fig. 12, let BC be a conducting spark channel elongating towards the right. If the channel is conducting the intensity inside it must be small; the tip B of the channel is therefore defined as the point ahead of which the intensity exceeds the value required to cause impact ionization in undisturbed air. Suppose that the field ahead of the channel tip in the direction of motion varies as shown by the curve, i.e. attains ionizing value at a point A ahead of the tip B by a small distance S. Let v be the velocity of advance of the channel; and v_1, v_2 the velocities, in a field of ionizing intensity, of carriers of like and unlike sign respectively to the electrode from which the channel issues.

When the edge A of the ionizing field arrives at any point in the trajectory of the channel, ion pairs will be produced. The ions of unlike sign to the channel will move towards it with velocity equal to or greater than v_2, and enter its tip after a time $t_2 \leqslant S/(v + v_2)$. Ions of like sign to the channel will move forwards with velocity v_1. There are now two cases to distinguish, according as the channel velocity v is greater or less than the velocity v_1 of the like ions moving in the same direction.

Case (1). $v > v_1$.—Since the forward velocity of the ionizing edge A is greater than the forward velocity of the like ions it produces, it is clear that no ions of like sign can ever get ahead of A. The time elapsing before a like ion produced by A is overtaken by B, is $t_1 \geqslant S/(v - v_1)$. If the like ions produce others during this period the supply should be so copious that A and B must be practically coincident and the end of the spark channel accordingly very sharply defined.

Case (2). $v < v_1$.—Since the forward velocity of the ionizing edge A is less than the forward velocity of the like ions it produces, these like ions will shoot ahead of A until they get into a field of intensity just sufficient to move them forward with the channel velocity v. These like ions ahead of A will not ionize and will weaken the field ahead of the edge A. The ionization in the region A–B will be due solely to the ions of unlike sign, which pass through this region very quickly.

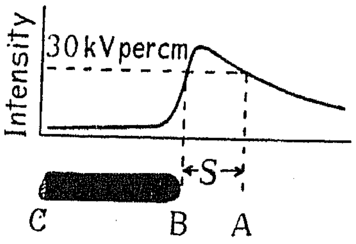

Fig. 12

Now it is generally believed that, at least in the initial stages of the spark discharge, the negative carriers are much more efficient ionizing agents than the positive carriers. Let us assume that the positive carriers produce no impact ionization whatsoever, so that the growth of the channel is entirely maintained by the ionizing action of the negative carriers. There are then two cases to consider:—

Positive channel.—Here v_1 will be the velocity of positive ions in a field of 30 kV per cm., i.e. of the order of 10^5 cm. per sec.[*] Experimentally-observed channel velocities are usually much greater than this, the range being from 10^6 to 10^9 cm. per sec. For a positive channel, therefore, the condition $v > v_1$ will usually be satisfied; the channel will have a sharp well-defined tip and ionization will occur by negative carriers in the range A–B moving towards the tip, exactly as pictured by Simpson.

Negative channel.—Here v_1 will be the velocity of an electron in a field of 30 kV per cm., i.e. of the order of 10^7–10^8 cm. per sec. This is well within the range of observed channel velocities, so that for a negative channel the condition $v < v_1$ is quite likely to occur. Since, by hypothesis, no ionization by positive (i.e. unlike) ions is permitted, a negative channel can extend only as long as the like negative carriers produce ionization, which, as shown above, occurs only when

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[*] See Reference (3).

[†] *Ibid.*, (9).

[‡] *Note added 18th September*, 1936.—This Appendix was sent to Dr. Schonland, who writes that a less detailed note reaching almost the same conclusions was published by him in the *Proceedings* of the Royal Society of South Africa recently. He had no copy to send to the author, who has been unable to obtain the *Proceedings*. This appendix has therefore been left as originally written. Dr. Schonland is now preparing a paper on the theory of the lightning flash which will be awaited with much interest.

[*] See Reference (49).

$v > v_1$, i.e. when the channel velocity exceeds the velocity of the negative carrier in the lowest ionizing field.

The difference between the foregoing theory and that of Simpson is only that a clear distinction is drawn at the start between the propagation velocity of the ionized channel and the particle velocity of individual carriers entering and leaving that channel. The result of this distinction is that a mechanism is found which permits the advance of a channel from a seat of negative charge—an experimental fact the possibility of which Simpson denied.

Discussion.

The most important deduction from the foregoing theory is that the propagation of a spark channel away from a seat of negative charge cannot occur with a velocity less than that of an electron in a field of ionizing intensity. A single reliable observation of a negative spark channel travelling steadily at a smaller velocity will make the theory untenable, at least in the simple form given above. The first matter is therefore the analysis of existing experimental data from this standpoint.

Table 7

Point-plane gap					
Gap length, cm.		25	50	75	100
Point positive	Time-lag	4·2	7·2	10	12·8 $\times 10^{-6}$ sec.
	Velocity	0·595	0·695	0·75	0·78 $\times 10^{7}$ cm. per sec.
Point negative	Time-lag	2·1	2·2	2·3	— $\times 10^{-6}$ sec.
	Velocity	1·19	2·27	3·26	— $\times 10^{7}$ cm. per sec.
Sphere-plane gap					
Gap length, cm.		—	50	75	100
Sphere positive	Time-lag	—	3·2	10·2	14·5 $\times 10^{-6}$ sec.
	Velocity	—	1·56	0·735	0·69 $\times 10^{7}$ cm. per sec.
Sphere negative	Time-lag	—	3·0	4·0	5·0 $\times 10^{-6}$ sec.
	Velocity	—	1·66	1·87	2·0 $\times 10^{7}$ cm. per sec.

The velocity u of an electron in air at normal atmospheric pressure in a field of ionizing intensity does not appear to have been measured, and there is also considerable uncertainty as to its theoretical value. On the assumption that the velocity of thermal agitation is small compared with the velocity imparted by the field, Townsend has deduced the expression

$$u = \sqrt{\left(\frac{2\lambda}{\pi}\cdot\frac{e}{m}\cdot\epsilon\right)} \text{ cm. per sec.} \quad . \quad . \quad (42)$$

where λ = mean free path = $5\cdot7\times10^{-5}$ cm., e/m = ratio of charge to mass = $5\cdot3\times10^{17}$ e.s.u. per gramme, and ϵ = field intensity, in e.s.u. per cm.

For an electron, therefore,

$$u = 0\cdot8\times10^{7}\sqrt{\epsilon'} \text{ cm. per sec.} \quad . \quad . \quad (43)$$

where ϵ' is the field intensity in kV per cm. The critical velocity v_1 for a field of 30 kV per cm. is therefore $4\cdot4\times10^{7}$ cm. per sec.

The experimental data on the velocity of spark channels are unfortunately very meagre. Allibone, Hawley, and Perry* have, however, published figures for the time-lag of spark-gaps subjected to impulse voltages, and these figures are analysed in Table 7. The experimental procedure was to apply to the gap an impulse voltage rising to its maximum value in 10^{-6} sec. and falling to half this value in 50×10^{-6} sec. The maximum value of this voltage was adjusted until the test gap sparked-over on 90 per cent of the applied impulses (minimum impulse sparking voltage), and the time-lag of sparkover was measured from cathode-ray oscillograms of the voltage across the gap.

The velocities are calculated simply as the ratio of gap length to time-lag. It is believed that the spark channel always develops from the smaller electrode, so that the velocities when the point or sphere is negative may be interpreted as the velocities of negative channels. On this assumption it will be seen that all negative-channel velocities are greater than 10^7 cm. per sec., while positive-channel velocities, with one exception, are all below this figure. For a given length of gap the negative channel travels 2–3 times as fast as the positive channels. No observed velocity is, however, as great as $4\cdot4\times10^{7}$ cm. per sec., so that on first consideration the theory propounded would appear definitely untenable.

It is, nevertheless, clear that velocities calculated from the time-lag of breakdown must be less than the actual velocity of the spark channel, since the time-lag includes an interval before the channel is formed during which the applied voltage is rising, and also the time-interval occupied by the return stroke. A reasonable allowance for these two intervals does not, however, increase the channel velocities to the required figure: but there are two other considerations which may explain the discrepancy.

First, it is by no means certain that the figure of $4\cdot4\times10^{7}$ cm. per sec. for the critical channel velocity is not too high. Some experimental figures obtained at low pressures and field intensities give an electron velocity of $1\cdot5\times10^{7}$ cm. per sec. when reduced to normal pressure and 30 kV per cm., and with this figure agreement would appear to be satisfactory. Second, the condition $v > v_1$ must be interpreted as referring not to the mean velocity but to the instantaneous velocity of the spark channel. Recent observations by

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* See Reference (40).

Schonland* have proved that the initial lightning leader stroke develops in a series of jerks or short steps, the propagation velocity over which is of the order of 10^9 cm. per sec., while the mean effective velocity of the leader between cloud and ground is only about $1 \cdot 5 \times 10^7$ cm. per sec. If a similar effect occurs in the spark discharge over smaller distances, the discrepancy between observed and theoretical velocities can be explained. In the opinion of the author the existing data on negative spark-channel velocities are insufficient either to prove or to disprove the theory put forward, which may therefore be accepted as a basis of discussion, pending further experiment.

In its travel from the edge of the ionizing field A to the tip of the channel B (Fig. 12), an electron must generate sufficient others to carry the current in the channel behind the tip. The number of electrons required can be estimated as follows:—Suppose that the voltage gradient along the channel is 50 volts per cm.; the velocity of an electron in this comparatively weak field will be $\frac{1}{2} \frac{e}{m} \cdot \frac{\lambda}{v_t} \cdot \epsilon$, where v_t ($= 1 \cdot 1 \times 10^7$ cm. per sec.) is the velocity of thermal agitation of electrons.† For the figures assumed, this electron velocity is about $2 \cdot 3 \times 10^5$ cm. per sec. If n is the number of electrons per cm.^{3} the current density in the channel is $n \times 1 \cdot 6 \times 10^{-20} \times 2 \cdot 3 \times 10^5 \times 10$ amperes. The current density in an *arc* channel is of the order of 100 amperes per cm.^{2}; this gives $n = 2 \cdot 72 \times 10^{15}$. If α is the ionization coefficient, one electron in travelling through d cm. will produce $e^{\alpha d}$ others by collision. Thus if *only one* electron per cm.^{2} of channel ionizes, we have $e^{\alpha d} = 2 \cdot 72 \times 10^{15}$, $\alpha d = 35 \cdot 6$.

The value of the ionization coefficient α at normal pressure is also somewhat of an unknown quantity. Townsend gives $\alpha/p = 0 \cdot 055$ when $\epsilon/p = 50$ (p = pressure in mm., ϵ = field intensity in volts per cm.), which at 760 mm. corresponds to $\alpha = 42$ in a field of 38 kV per cm. In a field of this intensity, therefore, an electron must travel a distance of $0 \cdot 85$ cm. to produce the required $2 \cdot 72 \times 10^{15}$ others. The time required to travel $0 \cdot 85$ cm. at the velocity of 5×10^7 cm. per sec., corresponding to a field of 38 kV per cm. [equation (2)], is $1 \cdot 7 \times 10^{-8}$ sec. There is no difficulty in finding this time when channel and electron are moving in the same direction. In the case of a positive channel, where the electrons move towards the tip, the time available is $t_2 = S/(v + v_2)$; with $v_2 = 5 \times 10^7$ sec. and $S = 1$ cm., the channel velocity v must not exceed $0 \cdot 59 \times 10^7$ cm. per sec. The assumptions which have to be made regarding the magnitude and extent of the tip field in order to account for fairly large channel currents are therefore by no means unreasonable.

APPENDIX II

The Capacitance of a Vertical Wire

The electrostatic capacitance of a wire of length l, diameter $2r$, in free space is approximately

$$\frac{l}{2\left[\log_e \frac{2l}{r} - 1\right]} \text{ e.s.u}$$

Its potential when carrying unit charge per unit length is therefore

$$2\left[\log_e \frac{2l}{r} - 1\right]$$

If the wire is hung vertically with one end just not in contact with the ground plane, its potential will be reduced by the influence of its image below the ground plane.

The potential at the mid-point of the wire at a height $l/2$ above the ground plane due to the image is

$$-\int_{l/2}^{l+l/2} \frac{dx}{x} = -\log_e 3$$

The potential of the vertical wire is accordingly

$$2\left[\log \frac{2l}{r} - 1\right] - \log 3$$

and its capacitance is therefore

$$C = \frac{1 \cdot 11\, l}{2\left[\log \frac{2l}{r} - 1\right] - 1 \cdot 09} \mu\mu\text{F}$$

To check the validity of this expression the earth capacitances of a number of vertical cylinders and wires were measured,* care being taken to eliminate irrelevant stray capacitances. The measured and calculated results are compared in Table 8.

Table 8

Height (feet)	3	6	6	6	6	10	100
Diam. $2r$ (inches)	2	8	6	4	$2 \cdot 75$	3	$0 \cdot 104$
Ratio $2l/r$	72	36	48	72	105	160	23 050
Capacitance ($\mu\mu$F) Calculated	$18 \cdot 6$	50	$43 \cdot 5$	$37 \cdot 6$	$32 \cdot 6$	48	199
Capacitance ($\mu\mu$F) Measured	17	54	$43 \cdot 5$	$35 \cdot 7$	$33 \cdot 3$	$48 \cdot 7$	207

APPENDIX III

Waves on Multi-Wire Systems†

Consider two parallel wires having return image circuits in the ground. When the currents in the wires are i_1 and i_2, their flux linkages per unit length are

$$\begin{aligned} \phi_1 &= L_{11} i_1 + L_{12} i_2 \\ \phi_2 &= L_{21} i_1 + L_{22} i_2 \end{aligned} \quad . \quad . \quad . \quad . \quad (44)$$

where the L's are the coefficients of self and mutual inductance.* Similarly when the charges on the wires are q_1 and q_2 their potentials are

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* See Reference (21).

† *Ibid.*, (50).

* By Mr. F. S. Edwards, B.Sc.

† See Reference (51).

$$\begin{aligned} e_1 &= p_{11}q_1 + p_{12}q_2 \\ e_2 &= p_{21}q_1 + p_{22}q_2 \end{aligned} \quad . \quad . \quad . \quad . \quad (45)$$

where the p's are the coefficients of potential.† Solving these equations for the charges, we get

$$\begin{aligned} q_1 &= K_{11}e_1 + K_{12}e_2 \\ q_2 &= K_{21}e_1 + K_{22}e_2 \end{aligned} \quad . \quad . \quad . \quad . \quad (46)$$

where $K_{11} = p_{22}/\Delta$, $K_{22} = p_{11}/\Delta$, $K_{12} = -p_{21}/\Delta$, $K_{21} = -p_{12}/\Delta$, and

$$\Delta = \begin{vmatrix} p_{11} p_{12} \\ p_{21} p_{22} \end{vmatrix} = p_{11}p_{22} - p_{12}p_{21} \quad . \quad . \quad (47)$$

K_{11} and K_{22} are the coefficients of capacitance, while K_{12} and K_{21} are the coefficients of induction.‡ Neglecting resistance and leakage, the induced e.m.f. $-\partial\phi/dt$ per unit length will equal the space rate of change of voltage along the wires; and the current leaving unit length of wire $(-\partial q/dt)$ will equal the space rate of change of current along the wire. Hence

$$\begin{aligned} -\frac{\partial e_1}{\partial x} &= L_{11}\frac{\partial i_1}{\partial t} + L_{12}\frac{\partial i_2}{\partial t} \\ -\frac{\partial e_2}{\partial x} &= L_{21}\frac{\partial i_1}{\partial t} + L_{22}\frac{\partial i_2}{\partial t} \end{aligned} \quad . \quad . \quad . \quad (48)$$

$$\begin{aligned} -\frac{\partial i_1}{\partial x} &= K_{11}\frac{\partial e_1}{\partial t} + K_{12}\frac{\partial e_2}{\partial t} \\ -\frac{\partial i_2}{\partial x} &= K_{21}\frac{\partial e_1}{\partial t} + K_{22}\frac{\partial e_2}{\partial t} \end{aligned} \quad . \quad . \quad . \quad (49)$$

Differentiating the first pair of equations with respect to x, the second pair with respect to t, and substituting the latter result into the former, we get

$$\frac{\partial^2 e_1}{\partial x^2} = A_{11}\frac{\partial^2 e_1}{\partial t^2} + A_{12}\frac{\partial^2 e_2}{\partial t^2} \quad . \quad . \quad . \quad (50)$$

$$\frac{\partial^2 e_2}{\partial x^2} = A_{21}\frac{\partial^2 e_2}{\partial t^2} + A_{22}\frac{\partial^2 e_2}{\partial t^2} \quad . \quad . \quad . \quad (51)$$

where

$$\begin{aligned} A_{11} &= L_{11}K_{11} + L_{12}K_{21} \\ A_{22} &= L_{22}K_{22} + L_{21}K_{12} \\ A_{12} &= L_{11}K_{12} + L_{12}K_{22} \\ A_{21} &= L_{22}K_{21} + L_{21}K_{11} \end{aligned}$$

Assume the wave solutions $e_1 = f_1(x - vt)$, $e_2 = f_2(x - vt)$, and substitute in equations (7) and (8). If f'' denotes double differentiation with respect to x, we get

$$\begin{aligned} (v^2A_{11} - 1)f_1'' + v^2A_{12}f_2'' &= 0 \\ v^2A_{21} \cdot f_1' + (v^2A_{22} - 1)f_2'' &= 0 \end{aligned}$$

For a solution of these equations to exist, the determinant of the coefficients must be zero. This condition gives the equation

$$v^{-4} - (A_{11} + A_{22})v^{-2} + (A_{11}A_{22} - A_{12}A_{21}) = 0 \quad (52)$$

whence

$$\frac{1}{v} = \pm\sqrt{\left\{\frac{A_{11} + A_{22}}{2} \pm \frac{1}{2}\sqrt{[(A_{11} - A_{22})^2 + 4A_{12}A_{21}]}\right\}} \quad (53)$$

The sign ± under the radical gives two numerically different velocities, unless the term following it is zero. The ± sign outside the radical shows that these velocities may be either positive or negative, i.e. that waves may travel in both directions along the lines.

Now the self-inductance of a wire of radius r at a height h above the true electrical ground plane (Fig. 13) is

$$L_s = 2\left(\log_e \frac{2h}{r} + \frac{1}{2}\right) \times 10^{-9} \text{ henry per cm.} . \quad (54)$$

the mutual inductance to the neighbouring circuit being

$$L_m = 2 \log_e \frac{a}{b} \times 10^{-9} \text{ henry per cm.} \quad . \quad (55)$$

The corresponding self and mutual potential coefficients, in (farads)$^{-1}$ per cm., are

$$p_s = 2 \log \frac{2h}{r} \times 9 \times 10^{11} \quad . \quad . \quad . \quad (56)$$

$$p_m = 2 \log \frac{a}{b} \times 9 \times 10^{11} \quad . \quad . \quad . \quad (57)$$

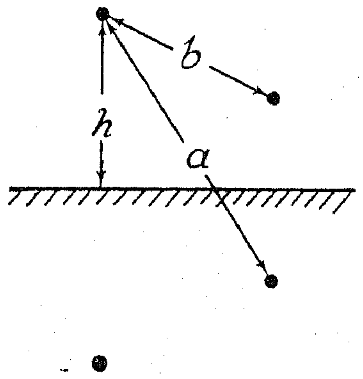

Fig. 13

The meaning of the expression "true electrical ground plane" is as follows. In calculating the capacitance and potential coefficients the electrical ground plane coincides practically with the surface of the ground, because the electrostatic field does not penetrate to any extent into the ground; the height h to be used in the formulae is therefore the actual mean height of the conductor above the ground. The effective electrostatic radius of the conductor may, however, be greater than its actual radius owing to the presence of an envelope of corona. For the inductance coefficients, however, the electrical ground plane will generally be well below the actual ground surface because the current penetrates a considerable distance into the earth. The height h to be used in the formulae is therefore greater than the mean height of the conductor above the ground.

Assuming, first, that the currents are confined to the surface of the wires, so that the factor $\frac{1}{2}$ in equation (54) due to the internal flux is absent; and second, that the electric and magnetic ground planes coincide, we can write $L_s = p_s/c^2$, $L_m = p_m/c^2$, where c is the velocity of light (3×10^{10} cm. per sec.). Using this relation together with equation (47) to determine the A constants, we find that $A_{11} = A_{22} = c^2$, while $A_{12} = A_{21} = 0$. In this case, therefore, equation (53) gives only a single velocity of propagation equal to the velocity of light. Now

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* J. H. Jeans: "Electricity and Magnetism," p. 446.

† *Ibid.*, p. 93.

‡ *Ibid.*, (94).

$$\frac{\partial e}{dt} = -v\,.\,f'(x - vt) = -c\frac{\partial e}{\partial x}$$

Substituting this relation into equation (49) and integrating, we get

$$\begin{aligned} i_1 &= cK_{11}\,.\,e_1 + cK_{12}\,.\,e_2 \\ i_2 &= cK_{21}\,.\,e_1 + cK_{22}\,.\,e_2 \end{aligned} \quad . \quad . \quad . \quad (58)$$

The cK coefficients are termed the self and mutual surge admittances of the wires. Solving these equations for e_1 and e_2 gives

$$\begin{aligned} e_1 &= Z_{11}i_1 + Z_{12}i_2 \\ e_2 &= Z_{21}i_1 + Z_{22}i_2 \end{aligned} \quad . \quad . \quad . \quad . \quad (59)$$

where

$$Z_{11} = p_{11}/c;\ Z_{22} = p_{22}/c;\ Z_{12} = p_{12}/c;\ Z_{21} = p_{21}/c \quad (60)$$

These coefficients are termed the self and mutual surge impedances of the wires.

The foregoing theory is quite general and can easily be extended to an n-conductor system. In the simple case when there is only one propagation velocity, the final result is a system of equations similar to equation (59), namely

$$\begin{aligned} e_1 &= Z_{11}i_1 + Z_{12}i_2 + \ .\ .\ .\ + Z_{1n}i_n \\ e_2 &= Z_{21}i_1 + Z_{22}i_2 + \ .\ .\ .\ + Z_{2n}i_n\,. \\ e_n &= Z_{n1}i_1 + Z_{n2}i_2 + \ .\ .\ .\ + Z_{nn}i_n \end{aligned} \quad . \quad (61)$$

These are the equations in general use for calculating waves on multi-conductor systems.

In the general case when the electric and magnetic ground planes and conductor radii are not identical there will be two velocities of propagation, say v_1 and v_2. We therefore write

$$\begin{aligned} e_1 &= f_{11}(x - v_1t) + f_{12}(x - v_2t) \\ e_2 &= f_{21}(x - v_1t) + f_{22}(x - v_2t) \end{aligned}$$

but integrating gives

$$f_2(x - vt) = \frac{1 - A_{11}v_{1,2}^{\,2}}{A_{12}v_{1,2}^{\,2}}f_1(x - vt)$$

Putting

$$b_{1,2} = \frac{1 - A_{11}v_{1,2}^{\,2}}{A_{12}v_{1,2}^{\,2}}$$

we can therefore write

$$\begin{aligned} e_1 &= f_{11}(x - v_1t) + f_{12}(x - v_2t) \\ e_2 &= b_1f_{11}(x - v_1t) + b_2f_{12}(x - v_2t) \end{aligned} \quad . \quad . \quad (62)$$

There are two pairs of coupled waves: one pair—f_{11} and b_1f_{11}—moves with velocity v_1, while the second pair —f_{12} and b_2f_{12}—move with velocity v_2. This result suggests some interesting possibilities. Thus suppose that b_2 is negative and that $v_2 > v_1$, so that f_{12} travels faster than f_{11}. The initial wave in conductor (1) is the sum of f_{11} and f_{12}, but, as the wave moves, f_{12} runs ahead of f_{11}, forming first a step in the wave-front and finally perhaps almost separating from it. In conductor (2) the initial wave is the difference of b_1f_{11} and b_2f_{12}. As f_{12} moves ahead a negative loop will appear in front of the wave, while its peak amplitude may actually increase instead of falling. Phenomena of this kind have, in fact, been observed.*

DISCUSSION BEFORE THE INSTITUTION, 7TH JANUARY, 1937

Sir George Simpson: It is now just over seven years since I had the honour of delivering the Kelvin Lecture on "Lightning,"* and, as the author has said, a great deal of knowledge about lightning has been gathered in the interval. The time is therefore ripe for another review of the position. As a meteorologist who has devoted a great part of his scientific life to a study of atmospheric electricity, I am naturally more interested in the first part of the paper, to which I propose to confine my remarks.

Soon after I had delivered the Kelvin Lecture the results of the new investigations which the author has summarized commenced to come in, and, as he says, the result was a marked turning-away from the theory of a thunderstorm which I had outlined, to the one propounded by Prof. C. T. R. Wilson. It was clear that the problem of the thunderstorm was not yet solved, and I was convinced that it never would be solved so long as we were confined to observations made at the earth's surface. I therefore set about considering methods of measuring the electrical field at all heights within the thundercloud itself. As the instruments would have to be carried by small balloons, it appeared hopeless to try to measure the actual field strengths; but a great deal could be done if we could determine the direction of the electrical field in different parts of the cloud. This was an easier problem, for the principle of pole-finding paper could be used. I sketched out a simple form of instrument and asked the staff of Kew Observatory to develop the idea. Mr. Scrase, who for a number of years had been in charge of the atmospheric-electricity work at Kew, took up the new problem with skill and energy, and he evolved a simple instrument, called the altielectrograph, which in its later form not only registers the direction of the field but also gives a rough idea of its intensity.

During the last 2 years, 70 balloons carrying altielectrographs have been released from Kew Observatory; 56 of the instruments were recovered, giving 47 satisfactory records. As the results of our observations set at rest several of the problems dealt with by the author, I feel that a short description of them should be attached to his paper.

In the first place, the majority of thunderclouds, if not all, have a concentration of positive electricity in their upper parts and a concentration of negative electricity in their lower parts; so that the thundercloud as a whole is of positive polarity. In the second place, there occur locally in the lower part of the cloud very concentrated masses of positive electricity. These concentrations of positive electricity are associated with the active centres of the storm from which the heavy rain, generally positively charged, falls, and where the lightning discharges are most frequent. There appear, therefore, to be two factors at work in a thunderstorm: one separates positive electricity from negative electricity in the upper part of the cloud, and the second, occurring only in the lower cloud and associated with heavy rainfall, causes intense concentration of positive electricity in relatively small volumes.

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* *Journal I.E.E.*, 1929, vol. 67, p. 1269.

* See Reference (51).

One's first inclination is to associate the former factor with the process of inductive charging, described by Prof. C. T. R. Wilson, and the latter with the process of charging by breaking of drops, described by myself. This conclusion is supported in the case of the positive charges in the lower atmosphere by the fact that they occur where the heavy rain and hail give evidence of strong ascending currents and where the temperature is above the freezing point.

The Wilson explanation of the separation of electricity in the upper portion of the cloud which gives to it its positive polarity is, unfortunately, not so satisfactory. In this portion of the cloud, as shown by our observations, the temperature is far below the freezing point, and there cannot be the rapidly-falling raindrops, with induced charges on their upper and lower halves, which Wilson's theory requires. Instead there are ice crystals which fall very slowly and tend to set themselves in a horizontal plane. Even if these crystals were sufficiently conducting to allow of induced charges on their upper and lower side, there could be no separation of the electricity in the manner described for rapidly-falling raindrops. In a paper which is being prepared, we are suggesting that the electrical conditions in the upper part of the cloud are of the same nature as those found in drift snow during blizzards in polar regions, in which strong positive fields are also found.

Mr. E. T. Norris: I suppose that the most important feature of lightning research in the last few years has been the discovery by Schonland of the stepped leader-stroke formation. Unfortunately this discovery has not been of much use to engineers, either quantitatively or even dimensionally; and the author finds this out when he comes to the mathematical part of his paper, where he is handicapped very badly by not knowing what assumptions to make. Just as the strength of a chain is no greater than the strength of its weakest link, so one may say that the value of a mathematical study is no more than that of the assumptions necessary for it. In his mathematical study the author clearly states what assumptions he has made, but he does not indicate which of those assumptions are probably or possibly justifiable, and which are pure supposition; and all three cases occur. Virtually the author, speaking as an engineer, tells us to pay very little attention to what he has said as a mathematician.

The characteristic which in the last few years engineers have concentrated on as being of most use to them in connection with the phenomena of lightning and thunderstorm formation is the value of the current in the stroke, and there have been, all over the world, a large number of efforts to measure what this current is. It is an important point in these investigations that most of the results obtained for the current (see page 7) are expressed in terms of an average or most frequent value. The maximum value, if given, is indicated as a freak occurrence so rare as to be of no practical importance. This seems to me misleading. To take an analogy, if we are considering the safety of a small boat on the Atlantic and for this purpose are measuring the height of the waves, if the maximum is 10 ft. the fact that the average wave is only 1 ft. in height is of no importance compared with the question how often the 10-ft. wave occurs. Similarly, in the case of lightning, if we get a current of 200 000 amperes the point that matters to the operating engineer is the likelihood of that stroke occurring on a given line; not whether it is one among 1 000 or 100 000 smaller and harmless surges. In other words, the maximum current should be expressed not in terms of other currents (in which case it usually appears sufficiently rare to be negligible), but in terms of time and space or, put more practically, in terms of strokes per annum per mile of line.

Dr. F. J. W. Whipple: The development of the work at Kew Observatory to which Sir George Simpson referred has taken two forms. The more important part is the soundings with balloons, by which the sign of the gradient of the potential at various heights has been determined; but a certain amount of information is also to be gleaned from other work which we have had in hand on the variation of potential near the ground as indicated by the variations in point discharge. What has struck me very much in this part of the work is that the variation in the potential gradient which takes place when lightning occurs is not such as one would have anticipated. Most of us, thinking of a charged cloud and of a discharge to earth, would say that before the discharge happens there is probably a very great potential gradient. One would expect that near the ground that potential gradient would be approaching the sparking point, and it had been suggested that the sparking point was reached just at the lower part of the cloud. We were surprised to find, however, that a lightning discharge frequently reverses a potential gradient, and that on the other hand it frequently makes the potential gradient stronger. It may happen that before the lightning flash occurs, the potential gradient is inappreciable; it is of the order, perhaps, of 1 000 volts per metre. When the flash occurs, the potential gradient increases to 3 or 4 times as much as before. This seems to me to be a paradox which requires explanation. The most common type of flash is one in which we have a negative gradient before the flash occurs, and this is actually reversed by the flash. There were 146 such flashes amongst the 562 flashes recorded at Kew in the period of 39 months covered by the Table.* The effect can be explained on the hypothesis that the electricity which is coming up from the ground—the induced current which is flowing up from all exposed points—produces, in the case where the field was negative beforehand, a positive charge in the air; when the lightning flash occurs the negative charge in the cloud disappears and we are left with a certain amount of positive charge in the intervening space, and that gives us a positive gradient underneath the cloud. Thus if we draw a diagram showing the signs of the electrical charges we ought not to confine it to the inside of the cloud; there is a great deal of free electricity outside the cloud.

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* "Analysis of Frequency of Lightning Discharges (July, 1932–December, 1934)," London Meteorological Office Geophysical Memoir No. 68 (1936), p. 12.

The other point that I wish to make is that we have not a steady field of a single sign underneath the cloud; continuous changes are going on. It is very seldom that the sign of the field in the lively part of the storm remains the same for as much as 5 minutes, and therefore the cloud has not a single polarity. The double theory to which Sir George Simpson has just referred, which supposes that there is electrical separation produced by two different processes, is most helpful in that it takes account of these continuous changes going on as the generating power changes and as the cloud drifts along overhead.

Mr. R. A. Watson Watt: In the years 1922–24 I was engaged in experiments by radio methods which demonstrated to my complete satisfaction that the base of a thundercloud normally had a negative charge and that its top normally had a positive charge. I should be sorry to have to accept the author's dictum (page 4) that these experiments "convinced few except the experimenters themselves." The experiments are still significant in the present discussion, because they also gave a good deal of information about the time relationships in the lightning-discharge process, and that information is not at all readily reconciled with the 1/50 standard specification about which Mr. Norris has expressed the same nervousness as I myself feel.

The kind of evidence which I have in mind is this. It is obvious that the radiation field of the atmospheric is a simple function of the acceleration of the electric moment which is destroyed in the lightning flash (the moment is twice the charge neutralized multiplied by the height through which it is lowered). If we integrate this function once, we are presented with a curve against time of the product of current and height in the lightning stroke; this curve shows that the current maximum is reached in a time of the order of a single millisecond—not a single microsecond—and I hope that this point will be given due weight before the 1-microsecond/50-microsecond standard impulse comes to achieve a standardized state of assumed infallibility. The integration can be verified as being appropriate to a lightning flash, because if we perform a second integration we come to the time curve of destroyed moment and that integration brings us into agreement, within a single power of 10, with C. T. R. Wilson's measurements of destroyed moment, and so identifies the atmospheric of distant origin with considerable certainty as being related to a lightning flash of the type directly measured at close range by Prof. Wilson.

I regard Appendix I as an outstanding contribution to the subject, of great originality. I refer to the part which deals with the mechanism of the discharge stroke, and which imports quantitative ideas into the first qualitative outline that Sir George Simpson gave us.

Mr. F. J. Scrase: One point to which I should like to refer concerns the electrical conditions between the base of a thundercloud and the ground. The author mentions the fact that although the field at the base of a thundercloud just before a flash can hardly be much less than 10 000 volts per cm., yet the field at the ground is known to be quite small. A long series of records at Kew shows that the field at the ground just before a flash is usually less than 100 volts per cm. and that it rarely exceeds 200 volts per cm. Prof. Wilson has suggested that such a large increase of field with height—amounting to something of the order of 30- to 100-fold—could be accounted for by the space charge set up between the cloud and the ground by ions of one sign produced by point discharge at surface objects. Estimates of the space charge obtained from actual measurements of point discharge seem to be of the right order of magnitude to produce the large increase of field. Moreover, the existence of the space charge appears to offer a satisfactory explanation of the fact that a considerable proportion of lightning flashes are accompanied not by a mere reduction of field but by a strong reversal. Thus there seemed to be good reasons for expecting a large increase of field with height to be a frequent occurrence below active thunderclouds. It was surprising, therefore, to find that the great majority of our soundings showed practically no increase at all between the ground and the cloud base. The largest increase recorded was barely threefold, and there was no indication of the intensity of the field at the cloud base approaching anything like the sparking value. Our failure to detect the large increase up to the cloud base may merely indicate that the intense fields at the base are extremely localized or that they develop very rapidly, in which case the chances of recording them by soundings would be small. On the other hand, it is possible that the upward current of ions from the ground into the cloud is nearly always sufficient to prevent the base from reaching the sparking value, and that the seat of the intense fields is at a higher level inside the clouds. The results of the soundings seem to bear this out.

Dr. E. H. Rayner: It may be useful to recall one of the experimental methods that Prof. Wilson used for his researches on this subject. He isolated a portion of the earth's surface, about the size of a billiard table, and measured its potential relative to the rest of the earth. The earth's surface was allowed to grow grass, etc., in normal fashion. One piece of apparatus which he used for the purpose was a mercury surface electrometer, which works on the principle that if a surface of mercury has another liquid, an electrolyte, in contact with it, the surface tension of the mercury alters and, if the tube is slightly inclined, the alteration of electrification at the surface will cause the mercury to move slightly backwards and forwards along the tube. He made the mercury itself, by means of photographic paper placed underneath it, record its own length along the tube. An apparatus of this kind, which gives results of fundamental importance, cannot work with anything like the quickness which is desirable for a good deal of lightning phenomena, and it seems to me that it may be worth while to consider applying a cathode-ray oscillograph to that type of observation.

In regard to the author's model showing a lightning flash between some hills, I think that in this case he would find a very considerable difference due to change of polarity. If we have a point sticking up an inch or two from a metal plate and a pointed high-voltage conductor above it and a few inches from it sideways, we can make the high-voltage discharge—it may be 1 or 2 ft. long—go either to the point or to the plate, according to the polarity.

I think that the phase of the lightning disturbance that the engineer has to deal with is of a different type from that which Mr. Watson Watt mentioned. We have to deal with events which happen a good deal more quickly than the records that he gets by his very beautiful methods.

The troubles caused by lightning will have to be overcome somehow, and every country in the world is trying to overcome them. In this connection it has occurred to me that if, on a length of line which gave trouble due to lightning, an overhead guard wire were provided in the form of a barbed wire, some effect—good or bad—might result.

Mr. K. J. R. Wilkinson: In calculating the impedance of a lightning channel, the author derives expression (4) by means which do not appear to me to be justified. This expression is based upon the assumption that if the capacitance per unit length of conductor be known, then a quantity called the surge impedance can be derived from the well-known relationship between this impedance, the capacitance, and the velocity of light. The simple relationship used is true, however, only for parallel-conductor pairs which are symmetrical about a line or plane parallel to themselves, and is unlikely to hold in the present instance where the plane of symmetry is at right angles to the conductor axes. Regarded from another standpoint, the expression for the impedance of a vertical conductor should take cognizance of radiation.

The paper contains evidence that neither the growth of a lightning channel, nor the speed of the return stroke, greatly exceeds one-tenth the velocity of light. I would therefore suggest that the capacitance and inductance of the lightning channel must play relatively unimportant parts in controlling its impedance, and that the impedance value is largely settled by ionization mechanisms, for which it would be incorrect to apply the term "surge impedance."

Dr. J. L. Miller: In his verbal summary of the paper the author suggested that the characteristic progression of the stepped leader stroke discovered by Schonland is a consequence of the nature of conditions at its tip. This seems to be a satisfactory explanation since, amongst other considerations, it probably makes it easier to account for the fact that later leaders of the same stroke are not stepped. At the same time there is an alternative explanation, namely that the phenomenon may be due to a cascading effect between various regions in the cloud during the formation of the leaders, and I should like the author's views on this point.

Two very important factors in the paper are the effects of the magnitude of the stroke current and of the rate of rise of stroke current. The first I have dealt with in another discussion.* The second is important because it may to some extent control tower-to-line flashover and because it has a bearing on the lengths of the fronts of the travelling waves on the line conductors and the rates of rise of voltage to which transformers may be subjected.

I do not completely agree with the author's conclusions regarding the influence of the rate of rise of lightning voltage in relation to the tower height and the tower surge-impedance. My criticism essentially is that while attention is drawn to the form and duration of the tower-top voltage no consideration is given to the magnitude of this voltage. This is evident from Fig. 7, where a time scale is given but where no ordinate scale is included, so that an important factor like the peak at 0·2 microsecond is not revealed. The calculation of the maximum tower-top voltage leads, however, to interesting results. The calculation can be made in various ways. For instance, a rate of current rise of 40 000 amperes per microsecond (Berger) or a rate of voltage rise of 5 000 to 10 000 kV per microsecond can be assumed and either of these assumptions can be employed in conjunction with Fortescue's, Schonland's, or the author's theories (page 12), but nevertheless all the calculations give voltages of the same order of magnitude. Actually all the answers are of the order of 350 kV to 700 kV, so that it would appear that low-voltage and medium-voltage steel-tower lines may flash-over irrespective of the tower footing resistance. Towers higher than the 100 ft. assumed (such as at river crossings) will be more prone to the effect because of the longer time-interval elapsing before the return of the reflected wave from the earth to the tower top, and I am of the opinion, therefore—and I think the author will agree with me—that this mechanism of flashover cannot be entirely left out of consideration. I find it difficult to see how the effect cannot but occur on lower-voltage lines and, although this view is not generally held, it may be the explanation for the flashover of towers having apparently low earth resistances.

Regarding the influence of the rate of rise of lightning current on the wave-front length of the resultant voltage wave on a line conductor, it is well to point out that there is some difficulty in reconciling the conflicting evidence available. On the one hand there is some evidence, obtained by direct methods not involving estimation from surges actually measured on the conductors, which shows that stroke wave-fronts persist for perhaps 2 or 3 microseconds. (In this connection it will be interesting when the discrepancy between the microseconds duration of lightning strokes and the milliseconds duration found by Mr. Watson Watt for atmospherics is cleared up.) On the other hand, both on systems without and with earth wires there is also evidence indicating the frequent existence of voltage waves on the line conductors steeper than 1 microsecond.

One explanation may simply be that the steeper wave-fronts on the lines are after all due to steep-fronted lightning strokes and that so far we have not succeeded in finding and measuring any of the latter by direct means (such as Berger's); in other words, we have not yet obtained sufficient results to correlate the measurements. Another explanation is that the measurements are incorrect, although so far as the transmission-line waves are concerned the error is almost certainly on the side of making the wave-fronts appear not so steep as they really are, because they are usually recorded far from the point of incidence and because the inherent trend of an oscillograph delay and potentiometer system is to give an apparent flattening of the recorded fronts.

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* See page 43.

Other explanations, however, suggest themselves. For instance, it is easy to correlate the existence of steep-fronted waves with slow-fronted strokes in the case of lines not having earth wires. The author shows that the maximum voltage tending to be attained on the line when directly struck is of the order of millions of volts, so that flashover will occur at a voltage which is only a fraction of this maximum, and at a time which is considerably less than the duration of the stroke front. Thus, even if the latter persists for several microseconds, the resultant wave-front on the line at the point of origin will be very steep.

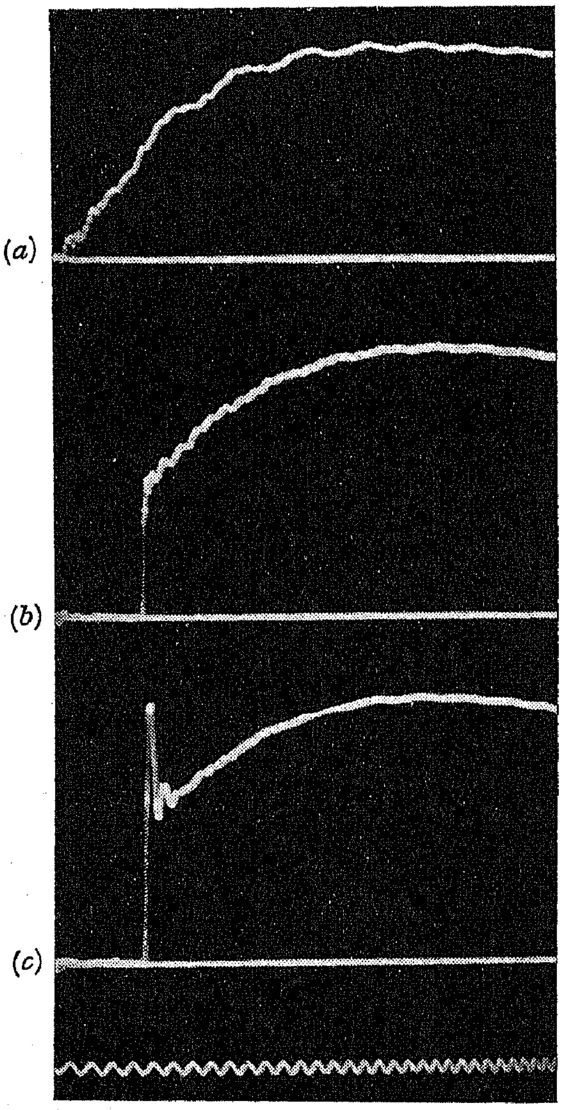

Fig. A

Frequency of time-calibration oscillation for all oscillograms, 5 000 kilocycles per sec. Transient deflection-sensitivities in (*b*) and (*c*) are the same, but the sensitivity is different in (*a*).

The case of lines with earth wires, the formation of the wave on the line being due to flashover from a stricken tower, gives rise to considerably more speculation. I suggest, however, that the effect may possibly be due to the fact that although in many cases the voltage at the top of a stricken tower may only rise relatively slowly, at the instant of flashover to the line the voltage here will rise suddenly because the current cannot change suddenly. Further, if it can be assumed that the tower has a separate capacitance to earth, the effect may be more pronounced.

I have illustrated the point experimentally as follows. An impulse generator was arranged to deliver a 3-micro-second-front wave on open circuit. The wave is shown in Fig. A(*a*), and represents the voltage at the top of a tower. This voltage was then allowed to flash-over across a second spark-gap (which represented the flash-over across a line insulator) on to an external resistance (one end earthed) of value such that it represented the surge impedance of a line. It was then found that the sudden change of impedance after the flashover caused the voltage wave across the external resistance to have a very steep front for a greater part of the total amplitude. This is shown in Fig. A(*b*). When a small condenser having a capacitance of value such that it represented the tower capacitance was connected across the generator terminals, the form of the wave-front across the external resistance was again different. It is shown in Fig. A(*c*). Adjustment of the constants can readily make the initial peak in this case merge into the main wave, thus giving a steep front for the whole amplitude.

When the generator was changed for one having a surge-impedance characteristic, so that possibly the lightning stroke was represented more accurately, and when still another resistance having one end earthed (to represent the tower earth-resistance) and another spark-gap were added, the same effect obtained except that the voltage corresponding to that shown in Fig. A(*b*) was steep for the whole amplitude.

I suggest, therefore, that the views I have here put forward may be the explanation of a mechanism whereby slow-fronted lightning strokes may give rise to steep-fronted travelling waves after insulator flashover.

Mr. W. M. Mordey: Some thirty years ago, I had to change a horse tramway in the Transvaal into an electric tramway, and I was asked whether I could do anything to prevent the very serious effect of lightning, in consequence of which the telegraph, telephone, and electric light wires all had to be open-circuited nearly every night at certain times of the year. The lightning was so continuous at these times that one could read quite small print without any artificial light. The trouble was overcome by fitting the poles with tapered iron finials at the top, and taking very great pains to earth those finials to the tram rails. This arrangement did away entirely with the need for opening the circuits.

Mr. C. E. Maguire: The salient points that emerge from the author's conclusions are that it is impracticable to insulate a line to withstand direct strokes on conductors without a flashover, and that an overhead earth wire which intercepts all direct strokes will reduce outages, provided that the tower footing resistance is sufficiently low. The desideratum, therefore, is that the earth wire shall be so designed as to intercept direct strokes with some degree of certainty, and, all reasonable precautions having been taken to ensure the lowest possible tower footing resistance, if direct strokes occur, to limit their intensity so that the resultant current passing to earth shall be reduced.

The author points out, on page 9, that any object giving a copious point discharge is particularly liable to be struck, and it is therefore not unreasonable to conclude that a sharply pointed earthed spike mounted on a tower does tend to reduce the chance of a direct stroke to a line conductor. The point discharge taking place continuously during the period of charging of a cloud will also have dissipated some of the energy, and therefore if a stroke does, nevertheless, occur its intensity will be reduced.

Some 3 years ago a line about 12 miles in length embodying this construction, without a continuous earth wire, was erected in Lincolnshire. Though other lines not so protected have been subject to insulator flashovers in the same district, this particular line has been entirely unaffected. One might carry that theory a little further and provide a continuous earth wire with barbs along its entire length. The barbs would provide many points for dissipation of the charge, and would also give some certainty that the earth wire would intercept the stroke.

Mr. E. Kilburn Scott: The virgin soils of Western Canada are rich in nitrates and wheat can be cropped year after year on them, without putting nitrogenous fertilizers into the soil. It is because lightning flashes have throughout the ages been fixing the nitrogen of the air, and rain has deposited it in the soil. In Norway, Germany, and elsewhere, nitrates have been made from the air in a similar way in large electric arc furnaces. The gas formed in the arc is nitric oxide, and it oxidizes, as it cools, to nitrogen dioxide, which is a red-brown gas with a pungent smell.

For many years I have held the opinion that the "balls" of ball lightning are masses of nitrogen oxides formed by the lightning. On various occasions I have published the idea, e.g. in a letter in the *Yorkshire Post* of the 20th October, 1923. In this paper under discussion there are three references to the colour of the ball: (1) ball lighting generally appears as a red luminous ball; (2) fireballs often leave a sharp-smelling mist, which appears brown; (3) floating fireballs have the red colour of meteorite tracks in the lower atmosphere. Surely these are confirmations. Quite obviously the balls are not electric, for the paper says "Floating ball lightning is not dangerous to human beings even when it appears in the middle of a group" but later it says "They heat the objects to which they are attached or along which they roll. They cause severe burns on the human body when they move over it." I suggest that the reference to the sharp-smelling mist and to the action of the ball on the body confirm the explanation I have suggested, namely that the ball is a mass of nitrogen oxides. Yet the author says "No satisfactory theory of ball lightning has so far been developed," and then goes on to refer to a speculation of Prof. W. M. Thornton "that a fireball is a mass of ozone produced by the discharge." In other words, Prof. Thornton goes part of the way; why not the whole way, I cannot understand, for ozone will not explain the colours and will not explain the burning effect, whereas nitrogen dioxide does explain them both. In the References and in some footnotes the author mentions articles and letters which have appeared in *Nature*, but he makes no reference to what I maintain is the very reasonable explanation that I gave in *Nature* of the 24th November, 1923.*

Mr. S. J. Rust suggests that earthquakes may be more closely connected with radioactivity than is generally accepted, and emanations may increase the potential of atmospheric electricity very considerably. The following are some interesting side issues on this question: (1) Attempts at increasing rainfall, or staving off hail, by gunpowder explosions (at which scientists have laughed) may be at times partially successful, because they upset the electrical equilibrium of the air. (2) At the beginning of the War, certain newspapers attributed the abnormal rainfall to the firing of cannon, but later there was dry weather without cessation of the firing. This may be explained if we attribute the rainfall to electrical disturbances effective at the beginning of operations, but reacting as the cannonading becomes habitual. (3) The old country notion that a meteorite is a thunderbolt has been ridiculed, but a meteorite rushing to earth at a time when an anticyclone has given hot dry weather, and an electrified atmosphere, may upset the electrical equilibrium sufficiently to produce a storm.

Mr. J. R. Mortlock: The theory postulated by the author covers a preponderance of negative strokes; but a few months ago McEachron† gathered a large amount of information with regard to lightning in general, and, as a result of that, he made the statement, based on field-measurement figures obtained by Wilson and others, that, provided all strokes and not merely strokes to transmission lines were considered, the numbers of positive and negative discharges were almost equal. On that basis there is considerable support for a combination of the two theories.

I am trying to find a simple explanation of why the leader stroke is followed by the main stroke, and I should like the author's comments on the suggestion that a part of the cloud charge is transferred into the leader channel, and that releases the equivalent ground charge at the earth's surface. The latter will start as a brush discharge from some point, and eventually leave that point to meet the leader stroke in space. This means that the current or potential at an earthed point will rise slowly with the growth of the brush discharge, and then finally, with the main discharge current or the combining of the leader and the main strokes, there will be a rapid increase followed by a slow decrease. If this proposition is reasonable, it will entail some modification of the theory propounded in connection with Fig. 6(*b*), in that this postulates a voltage of definite magnitude being suddenly impressed through a switch on to an unenergized impedance. At the same time, even if the proposed theory is entirely correct it does not prevent the practical application of the information available, because the two theories [Figs. 6(*a*) and 6(*b*)] give the same current, and it is on the basis of current that the effectiveness of various methods can be assessed.

A recent paper by Bewley‡ correlated current records from many sources and over many years, and related the magnitude of the current to the frequency of occurrence. From such records as these it is possible to assess the advantages which can be obtained from lightning protection. As an illustration, I recently had occasion to assess the effectiveness of earth wires run over a 33-kV outdoor substation with a clearance of about 6 ft. between the busbars and the earth wire and a span of 200 ft. between earthing points. The figures showed that with an earth resistance of 10 ohms the percentage of direct strokes which would probably cause a flashover to earth on any one phase was 1, with an earth resistance of 15 ohms it was 5, and with an earth resistance of 30 ohms it was 35. The percentage of direct strokes which would cause a double phase-to-earth flashover was negligible.

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* *Nature*, 1923, vol. 112, p. 760.

† K. B. McEachron and W. A. McMorris: "The Lightning Stroke: Mechanism of Discharge," *General Electric Review*, 1936, vol. 39, p. 487.

‡ L. V. Bewley: "Flashovers on Transmission Lines," *Transactions of the American I.E.E.*, 1936, vol. 55, p. 342.

On page 20 the author refers to expulsion gaps as being made of fibre. The modern expulsion gap is a composite structure, with a fibre liner. Its weathering properties are generally satisfactory.

Sir Charles V. Boys (*communicated*): The author refers to flashes from the upper side of a thundercloud to the clear air above, which have been seen simultaneously with the flashes to earth below. I have watched thunderstorms night and day whenever I have been able for about 70 years, and I have seen this type of flash only once—that was about the year 1876 at my old home at Wing in Rutland. This was a magnificent display and it is sufficiently described in my first letter on "Progressive Lightning."*

I may perhaps mention that at a meeting of the Physical Society in 1889 I stated in a discussion on lightning† that very commonly the lightning flash was repeated on exactly the same path with great rapidity. To my surprise this very obvious phenomenon was doubted by those present, and this led Dr. Hoffert to waggle a camera at the first opportunity and thus obtain his famous photograph‡ which confirmed my assertion in every particular.

My next contribution to the study of lightning was made in the year 1900, when as the result of a little mental arithmetic I concluded I could move a camera lens sufficiently quickly to distort the true form of a lightning flash by moving it appreciably while the lightning was travelling between the cloud and the earth, for different parts would be photographed by the lens in slightly different positions. But unless the true form was known no information would be obtainable from the distorted photograph. If, however, two lenses were mounted on a wheel so as to move in opposite directions the distortions would be opposite in kind and the difference double of either in amount, and so the rate of travel and direction should be ascertainable. If the flash were to travel at the speed of light, even this should come within the power of the instrument. I therefore made the first instrument myself, but it was not for 28 years, until I had taken it to Tuxedo Park, New York, at the invitation of Mr. Loomis, that I caught my first flash. The two images were visibly different in shape and I knew that the method was workable. It was with a number of these cameras made in S. Africa as well as with my original one that Dr. Schonland and those working with him obtained their splendid results.

I then designed the drum camera,* and Mr. Loomis had the first made by Messrs. Ross, Ltd. This is of much greater power and is preferable in every way to the original instrument. Mr. Loomis lent his instrument to the electrical engineers at Pitsfield, and they obtained confirmation of Dr. Schonland's discovery of the leader stroke, but by a much clearer photograph than any which he had obtained. He now has the second drum camera made by Ross, and in America they have made more on the same system. I am glad that both in S. Africa and in America those using these cameras have given credit to me by calling them "Boys lightning cameras." The present author allowed me to test the first drum camera upon his million-volt spark at the Metropolitan-Vickers laboratory at Manchester. This I wished to do, not expecting to find any evidence of the time taken to travel the 5 ft. to the ground plate, but only to see whether the behaviour of the instrument was good. It seemed perfect. So perfect is it that it is possible with this to watch the lightning crawl across the sky and to measure its rate of travel. Indeed, even if lightning moved several times as fast as light it would still appear to be not instantaneous.

Dr. Schonland's other discoveries of the initial stepped leader and the nature and origin of the branches were made with the original form of Boys camera.

I now have my original camera at my house near Andover, but it seems hopeless to get any results there, as in the 23 years since I went there I have not seen a single compact storm. The lightning is always here, there, or anywhere, and there is no direction in which to point the camera with any expectation of success. It was very different in London, and also at Wing in Rutland. It is possible that some member of The Institution who lives in a favourable locality might like to borrow this camera, and I would willingly lend it on the understanding that in the event of my death it should be passed on to the Science Museum, South Kensington, to which I have promised it, to go with the first progressive lightning photograph ever taken (Tuxedo, 1928), which they now have.

The author refers to the rare phenomenon of ball lightning; what appears to me to be a most important observation in connection with this is given in a letter to the *Daily Mail* of the 3rd October, 1936, by Mr. W. Morris of Dorstone, Hereford. He had unintentionally set a trap for it, and he succeeded in catching and killing it! He saw the often-described red-hot ball come down and cut a telephone wire, burn the window frame, and dive into the water in a water butt, which "boiled for some minutes." When it was cool enough, he went to see what he could find in the butt, and found nothing. The importance of this observation is that for the first time, so far as I know, a measure of the energy in the red hot ball has been roughly found. I have ascertained that the ball appeared to be of the size of a large orange, and after 20 minutes the water was too hot for Mr. Morris to put his hands into it. The amount of water was about 4 gallons.

Mr. Goodlet states that "more knowledge of lightning has been gained in the last seven years than during the previous seventy." I think he indicates clearly enough that much of this is the direct result of the use of the Boys lightning camera, which ascertains facts apart from all theory or imagination.

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* *Nature*, 1926, vol. 118, p. 749.

† *Ibid.*, 1889, vol. 40, p. 71.

‡ *Proceedings of the Physical Society*, 1889, vol. 10, p. 176.

* *Nature*, 1929, vol. 124, p. 54.

Mr. F. E. Lutkin (*communicated*): Several speakers have referred to the current/time characteristic of each stroke of the lightning flash, and it is thought that the following remarks may be of interest in this connection. Photographic records of the wave-form of the radiation from lightning flashes (i.e. radio atmospherics) made from 1931 onwards at the Radio Research Station, Slough, have shown one predominant feature of the wave-form to consist of a heavily damped high-frequency oscillatory train of some 4–5 cycles in addition to the longer-duration pulse mentioned by Mr. Watson Watt. Prior to observations made during the summer of last year it had been impossible to determine whether this train was due to current variation during the discharge process or whether it resulted from oscillations impressed on the radiation in course of propagation. Some records taken on very near flashes during the summer of 1936 do show the presence of this oscillation very near the source, and I think the explanation of its production to be that the "return stroke" really consists itself of successive current pulses up (or down) the ionized channel formed by the leader.

The predominant frequency of this oscillation lies between 5 000 and 12 000 cycles per sec. and is of interest in connection with the 50-microsecond pulse discussed, since repetition of such pulses after a further 50-microsecond interval would give a 10 000-cycles per sec. alternation. Two or three "current pulses" in the "stroke" would remove the discrepancy between the 10-coulomb "standard charge conveyed" and Prof. Wilson's "20 coulombs or more"; further suggestions regarding this mechanism will be found in Fig. 14 of Reference (14) in the paper.

It would seem, then, that the engineer must consider the effect of some 3 or 4 pulses of "standard discharge current" within a millisecond, not forgetting possible additional effects in line or associated equipment resonant to about 10 kilocycles per sec.

Mr. J. E. Taylor (*communicated*): The paper deals with a point having an important relation to the physical side of the subject, with which I feel impelled to register disagreement. The idea is perpetuated that ionization of the air at and near the ground level is produced by point discharges from ground objects in the electric field of a thundercloud. Is there any real basis for this assumption? If we start with air which is sensibly clear of ionization it surely requires a potential gradient far greater than any which have been observed to start such discharges. Moreover, experiment shows that until there is visible luminosity at a point in such conditions no discharge occurs. What of the electric field surrounding a 132-kV transmission line; is it seriously contended that, in the absence of corona, there are such point discharges, say, from points on the pylons near to the conductors, even though that field is far more intense than those due to thunderclouds? I do not deny that there is considerable ionization of the air beneath a thundercloud, doubtless due mainly to heavy ions, and I accept the proposition that this ionization may be largely instrumental in determining the path and character of a lightning flash, but is it not much more reasonable to suppose that this ionization proceeds from the source of electrical disturbance up above and that, so far from points generating ions at ground level, they are actually collecting them and reducing the ionization? This view is supported by tests I have made during the past summer, showing that conducting points are not essential to get the effect of a current flow from ground to air. On replacing a pointed conductor by a brass ball the flow of current was still observed, though possibly of less magnitude—as might be expected. The experiments are not complete, but it is hoped to get a quantitative comparison when more thunderstorms are available. The rather rare phenomenon of St. Elmo's fire is very generally quoted in substantiation of the view generally held, but the descent of electrified air (or heavy ions) in sufficient volume affords, in my view, a much more satisfactory explanation of this effect. The idea of point discharges as a cause of ionization is usually attributed to C. T. R. Wilson, I think quite unfairly. On referring to his original paper* it will be seen he advances the idea in a guarded and tentative manner as something which might be considered.

The falling-drop theory has never appealed to me, for a number of reasons, but perhaps chiefly because it does not account for lightning discharges on clouds from which no rain is falling, nor for lightning during snowstorms. Moreover, I see no reason why, if the theory were correct, it should not be possible to produce miniature lightning flashes in the laboratory on this plan.

The last word on lightning theory has not yet been said, but there is no doubt that we are getting nearer the truth and that an important forward step has been taken in recognizing that the flashes are a phenomenon of volume electrification having little in common with surface discharges.

Mr. W. Wood (*communicated*): The paper apparently does not deal with cases arising out of inductive effects caused by discharges from a cloud to a cloud, or from a cloud to earth, in the neighbourhood of transmission circuits or (what I have particularly in mind) communication circuits or signalling circuits of various kinds, such as those used by the Post Office and the railway companies. After a severe thunderstorm has occurred along a railway line it sometimes happens that trains are slowed down owing to the inductive effects from lightning discharges; the steep-fronted transients induced along the lines may burn out and otherwise affect the delicate signalling apparatus and telephone control systems. In my experience it rarely happens that lightning terminates on pole lines of this kind, and the problem for lightning-arrester designers is to devise apparatus to take care of the transients caused by the electrostatic effects of the stroke. The usual method is for the lightning arrester to take such a form as enables charges to be conducted or led to earth before the delicate parts of apparatus are affected, added protection being given by the use of choke coils suitably disposed in the circuit. The induced surge coming to the choke coil is reflected as it meets with the very large impedance. I should like to ask the author to give his view and, if possible, a mathematical treatment, of what takes place when a severe cloud-to-cloud discharge occurs over an earthed signalling circuit or, say, a telephone metallic loop circuit.

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* *Philosophical Transactions of the Royal Society*, A, 1920, vol. 221, p. 24.

[The author's reply to this discussion will be found on page 55.]

SCOTTISH CENTRE, AT GLASGOW, 8TH DECEMBER, 1936

Prof. M. G. Say: Is it quite certain that upward air currents are responsible for the separation of positive and negative charges in clouds? The whole matter seems to be very speculative, and the fact that two diametrically-opposed theories can be put forward and accepted within a few years is evidence of our ignorance: I refer to the hypotheses that make the base of the cloud respectively positive and negative. It would not surprise me in the least to hear of a brand new theory on quite a different basis which would explain observed phenomena equally well. Has it been found possible to investigate the electrostatics of clouds on a reduced scale in the laboratory? It would seem that considerably more experimental data of this kind are necessary to eke out observations on the natural scale.

The charge conveyed by a powerful lightning stroke is given as being of the order of 10 coulombs. This is only a small fraction of the coulombs taken from a motor-car battery to start the engine, but the associated voltage is of quite a different order. This prompts the suggestion that the bursting of trees and wood poles by the passage of a lightning discharge may be due to effects additional to, or other than, the tendency for an arc to enlarge its cross-section if confined to a small-bore tube. In the first place, when an electric arc discharge reaches a conductor it becomes therein a non-visible current flow, and the phenomena associated with arcs in gases may not apply. Can the author say what objections there are to the supposition that the bursting of trees, etc., may be due to the production of steam internally at high pressure? When the surface of such conductors is wet, it provides a lower-resistance alternative path, reducing the current within the tree and therefore also the steam production.

The very high voltages that may be produced on transmission-line conductors suggest that corona loss would be a powerful dissipating agent. I do not think the author referred to corona except in connection with a change in the surge impedance produced by the effective increase in the conductor diameter. Corona is often referred to as providing an automatic safety-valve for over-voltages, and I should like to know whether there is any foundation for this view in connection with lightning surges.

To one who has never seen "ball lightning," the description of the phenomenon given by Dr. Brand reads like a book of mediaeval magic. Evidently undue scepticism may be as misleading as undue credulity. I look forward with great interest to seeing one of these remarkable objects.

Mr. C. H. A. Collyns: There were two occasions during a lightning storm when a meter and cut-outs in a consumer's premises on our system were completely wrecked, and these premises were more than a mile away from any overhead line, and moreover were fed through double-wound transformers. If the author can suggest an explanation for this, I shall be very glad.

With regard to the effect of reflection waves in lowering the voltage, there is a very good argument here for the use of short spans, and steel poles rather than wooden poles. If wooden poles are used one must run a conductor down each pole instead of only earthing poles at intervals of not more than ¼ mile, as required by the Commissioners' Regulations; but it seems to me from the paper that in our case an overhead earth wire will not prove very effective, as the majority of our lines operate at only 11 000 volts and are without any resistance to the voltage encountered during lightning flashover. The voltage at the front of the wave must be very much in excess of 11 000 volts. On the other hand, our experience goes to prove that an overhead earth wire minimizes lightning troubles even with lines of 11 000 volts. This is the first time I have heard of a double overhead earth wire, but it seems quite a good scheme from a theoretical point of view. I am afraid, however, that the cost would make it almost impossible for rural development.

Mr. J. Eccles: It is difficult to visualize a cloud carrying such an intense charge in such a local area as is necessary to produce a lightning stroke. Whilst it is quite true that the currents only last for an infinitesimal length of time, the extraordinary thing is how a cloud of reasonable magnitude can sustain a charge which produces instantaneous currents of the order of 100 000 amperes. What happens when the leaders are being formed? Is the cloud busy collecting a charge from more remote strata to replace that being dissipated in the leader path?

The author pictures lightning as a constant-current phenomenon, and if this is a true picture there must be almost unlimited voltage in the cloud itself. What is the magnitude of the voltage? If the voltage is 10 million volts on a line which has a surge impedance up to 500 ohms, what must the voltage employed initially be in order to penetrate several kilometres of space between the cloud and the line?

The base of the cloud appears to be at a negative potential relative to the earth. Would it be a feasible suggestion to insulate the earth wire from the tower and raise it to a positive potential above the earth, so that, in the event of a much higher voltage being impressed on this wire, it would spill over? Would such an arrangement be of any assistance in persuading the lightning to attack the earth wire, which is now charged more positively than the earth, and might, therefore, form a point of attraction for the downcoming stroke?

Prof. C. T. R. Wilson: My own measurements relating to lightning discharges have been made mainly with the aim of obtaining quantitative information about the thundercloud regarded as a great electric generator.

There can be little doubt that the potential differences in thunderclouds attain values of the order of a thousand million volts and that the average quantity discharged in a lightning flash is of the order of 10 coulombs.

Any picture of a thundercloud is, I think, incomplete unless the conducting upper atmosphere is included. This is generally recognized in connection with the possibility of the maintenance of the fine-weather potential-gradient by thunderstorms. But in my view the conductivity of the upper atmosphere must also play a quite essential part in the determination of the whole electrical behaviour of the thundercloud, including the production of lightning discharges.

Mr. A. Erskine: A few months ago we in Fifeshire had rather an interesting experience with an overhead line which was subjected to a direct lightning stroke. The line to which I refer was carried on wood poles and connected our power station to a substation some considerable distance away. After a short but severe lightning storm, one of the wood poles was found to have become fractured about 6 ft. from the ground level and a transformer in the substation fully 2 miles away had broken down to earth near the centre of its e.h.t. winding, a part, I believe, which is severely stressed under such conditions. I should be pleased to have the author's comments on such a transformer failure.

Mr. P. Butler: It is disappointing to find that Section (1) (*e*) refers only to direct lightning strokes on either the supporting towers or the mid-spans, whereas in actual practice the proportion of direct strokes on a distribution system is comparatively small. It is our opinion that on the lower-voltage systems, i.e. 11 kV and under, the destructive effects of a direct stroke cannot be guarded against, as invariably one or more of the line insulators are shattered and an outage occurs. According to the Dumfries County Council engineers, most lightning trouble is due to induced surges set up by a direct stroke in the vicinity, or by a heavily charged thundercloud. The Dumfries undertaking have 600 miles of 11-kV lines, and during 1936 they had 12 thunderstorms but only one direct hit. In consequence of the secondary disturbances, however, 140 fuses failed during that period. These fuses protect separate transformers erected on either the main lines or the spur lines, varying from ½ mile to 1½ miles in length. On some of the spur lines there are as many as eight transformers connected in, of sizes ranging from 5 to 15 kVA. After a storm the lines and transformers have been carefully examined but no signs of a flashover or any other material damage has been observed. If the author could supply an answer to this serious problem it would be of great assistance to every distribution engineer.

With regard to the question of earth wires on the top of towers, it has been suggested by one authority that the best place for these earth wires is on the underside. Some distribution engineers do not like to have the earth wire on top owing to the danger of breaks and consequent short-circuits between lines. The author mentions the use of the Petersen coil; it should be pointed out that this is not a protection against lightning strokes but purely a method of ensuring continuity of supply in case one line breaks down to earth. I was rather interested to find that these coils are being used in Russia; the first transformers supplied to Russia had earthed neutrals, and it is presumed that these have been modified in order to allow the use of Petersen coils. The author also suggested the use of gaps; we have carried out some experiments in this direction with fibre tubes, but we could not get consistent results. If the author could give any particulars of the gaps he recommends it would be of assistance.

In Section 1 (*d*) it is suggested that certain identifiable localities receive an undue proportion of lightning strokes. This is very difficult to prove in actual practice. In one case in particular, where there is a large lead deposit, the disturbances are relatively few, and this is entirely contradictory to what the author suggests.

Mr. H. C. Babb: If in order to ensure protection of overhead lines against lightning we have to put in all the extra equipment recommended in the paper, the line cost, especially for the lower voltages (up to 11 000 volts), is going to be so high that it will be almost as cheap to put in cables.

I should like to ask the author how he proposes to provide the larger clearance in the centre of the span if the earth wire is carried above the power wires. It seems to be rather a difficult job under the existing regulations. Regarding the discharge from the centre of a span, we have had reports of such discharges on one or two occasions, but in each case there was heavy rain at the time, and the water was falling in large quantities from the centre of the spans. We concluded that this shortened the clearance and thereby caused the flashover. In view of what the author says, however, this might not have been so.

[The author's reply to this discussion will be published later.]

NORTH-EASTERN CENTRE, AT NEWCASTLE, 14TH DECEMBER, 1936

Mr. F. C. Winfield: In dealing with a problem of this type there are as a rule in general engineering two distinct lines of attack which proceed side by side. The first is the cut-and-try method of practical engineering, often wandering a little blindly but being corrected continuously by experience. The other is the more exact method of scientific investigation. In this subject of lightning both lines of attack have been greatly hampered not merely by the abstruseness of the problem but also by the tremendous uncertainty which attaches to all natural phenomena of this type. The practical engineer finds so many variables and so great a difficulty in recording exactly what has happened that he is unable to fix his experience clearly and profit by it. The scientist has been hampered largely by the very immensity of the problem and the grave costs and time involved in the provision and use of suitable research plant.

Until a few years ago the actual progress made on either side was rather disappointing. Lightning arresters of very doubtful quality, and the Petersen coil, represented practically the whole useful contribution. The situation now seems to me very different. In my Chairman's Address to this Centre[*] last year I suggested that the old haphazard attack on this abstruse problem was now giving way to the method of statistical examination and recording coupled with extensive laboratory research and international collaboration; and I think it a reasonable prophecy that within the next 5 years lightning will be under practical control.

I am able to say from my own experience that many of the points made by the author are borne out in practice. One in particular I would mention, and that is the desirability of a low tower footing resistance for overhead lines. This is very well appreciated now amongst supply engineers, and I think that if every tower footing could be brought within the 5-ohm figure which the author mentions our problem would be very largely solved. In engineering practice we are concerned with commercial solutions which give reasonable service rather than absolute immunity from trouble. All of our important circuits are in effect duplicated, and it is therefore not necessary to eliminate lightning outages entirely on overhead lines; but it is very necessary to reduce their present number.

What is the range of magnitude of lightning which we may anticipate? From my own experience it would appear that this range is very large, and that, without separating direct strokes from indirect effects, the percentage of lightning effects giving really high voltages is quite small. Experience indicates that the number of line outages per mile of line in this country, at any rate, varies inversely with the voltage, i.e. as the level of insulation rises with the voltage of the line more and more lightning effects are withstood without line outage. It is highly probable that interruptions from this cause on lines of the order of 250 000 volts would be rare.

The author refers to the value of double earth wires and counterpoises, and I should be glad to know whether, taking the present grid 132-kV lines as an example, he considers that greater immunity from lightning would be achieved by providing double earth wires or adding a counterpoise to the present single earth wire. My own impression is that double earth wires, if properly located with respect to the lines, would achieve the better results. The author refers to de-ion gaps and arc-suppression devices, and mentions that there are certain limitations in their application. I have found these limitations very serious, particularly as regards the minimum limit of current specified.

Another matter on which I should like the author's view is the relative intensity of lightning effects on overhead lines in this country and abroad. My general impression is that the severity of lightning in this country is less than is found on the Continent or in America. His reference to the increased number of flashovers between clouds in tropical countries suggests a greater immunity abroad than appears to be the case, unless the number of storms is very much greater. On the other hand, I should like to know whether the existence of storm clouds at a low level in this country is liable to reduce the magnitude of the voltages produced.

Mr. James Dickinson: On page 9 the author attributes the bursting of tree trunks to the high pressure set up by the arc of the leader stroke when constrained to flow in a tube with a bore smaller than the natural cross-section of the arc in free air. I have been accustomed to explain the shattering of such moist semi-conductors by the analogy of the ordinary steam boiler. The sap of the tree trunk, for example, is instantaneously evaporated by the heavy I^2R loss resulting from such a high current flowing through the semi-conductor. The steam pressure rises almost as rapidly, with the result that the cylinders of wood and bark literally burst like an ordinary steam boiler.

It is appropriate to call attention to a different type of surge generator, employing high-voltage cable instead of a battery of condensers, which is readily portable to points on actual transmission lines, has much greater stored energy than the usual type, and will faithfully imitate the effect of lightning. Fig. B shows clearly the scheme of such a surge generator. There are three special cables (C_1, C_2, C_3), each for 350 kV direct voltage, connected in accordance with the Marx circuit. The construction both of the cable and of the generator is outlined in detail in paper (1) referred to at the end of my remarks. The cables are slowly charged to 350 kV(max.) by means of the two thermionic valves shown on the left, which also charge the auxiliary cables C_0 to the same maximum voltage. The spark-gap F_0, set for the required voltage, discharges and thereby acts as a trigger to F_1 and F_2. The three cables are thus connected in series with the spark-gap F_3, across which approximately 97 per cent of the sum of

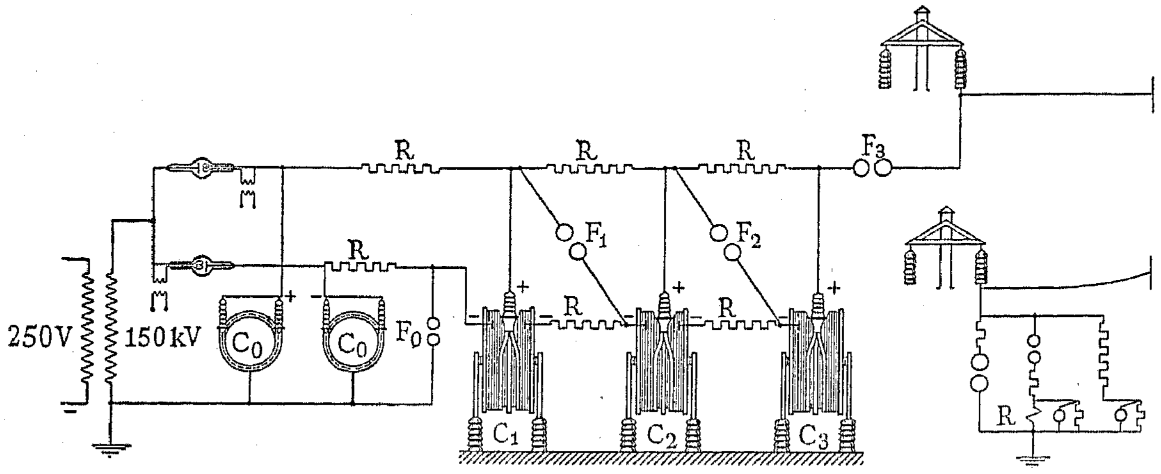

Fig. B

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[*] *Journal I.E.E.*, 1936, vol. 78, p. 34.

the voltages of the charged capacitances will appear. In the case of concentrated capacitance the voltage loss is small, but the capacitance under consideration is distributed, and it is shown that there is a voltage loss, due to the resultant surge impedance of each cable-condenser (impedance Z_k), of just over 3 per cent. Fig. C shows a simplified diagram of the surge generator. Cathode-ray oscillograms show that the surge wave generated by the distributed-capacitance cable generator is similar to that of the multi-step concentrated-capacitance type.

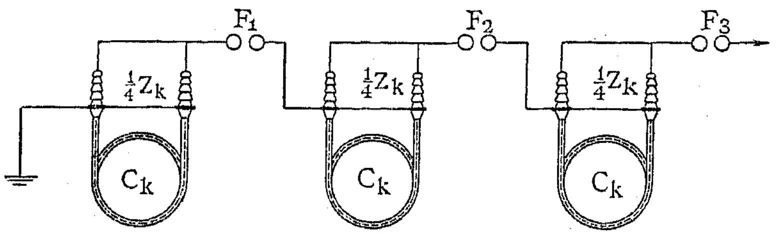

Fig. C

In one case described and illustrated in paper (2) below a good deal of damage was done by lightning to a concrete pedestal at one foot of a pylon. The conductors and insulators exhibited no damage on this particular tower. A house near by was struck and damaged at the same time. In the nearest power station, about 6 miles away, there was damage on the red phase, and two clearly-defined discharge paths could be seen afterwards.

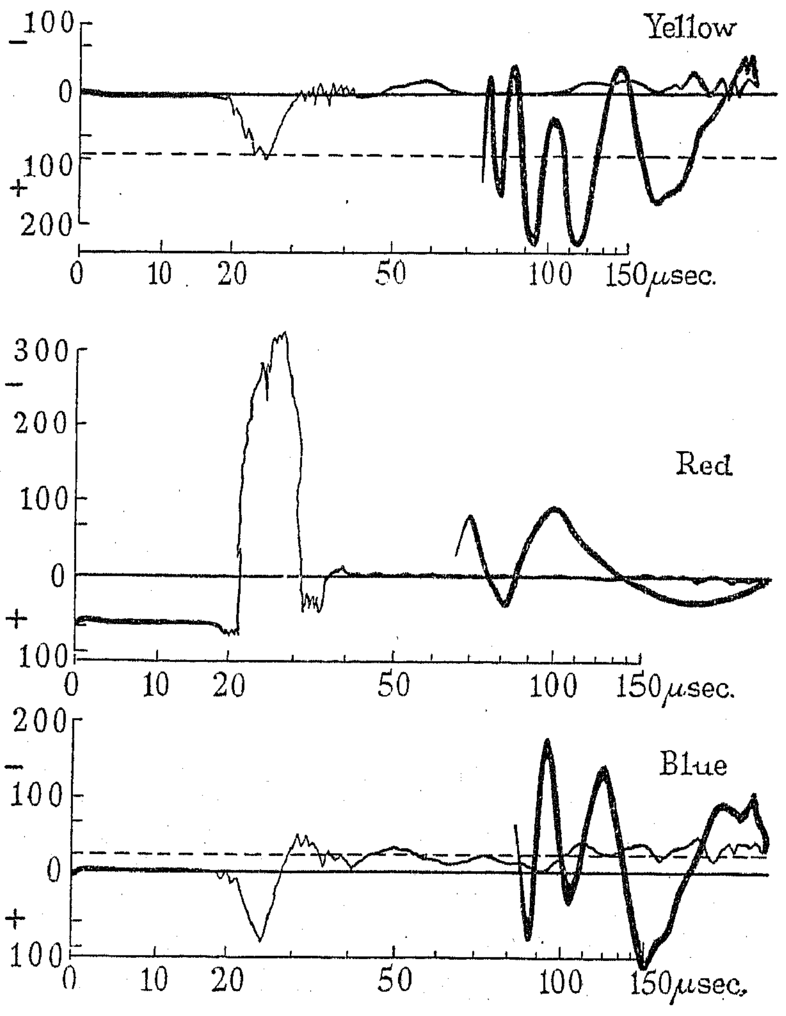

Fig. D

Ordinates indicate peak values in kilovolts. Normal phase voltage of network, 80 kV.

The surge-voltage oscillogram in Fig. D shows, in the early stages, an insignificant but sudden voltage variation with respect to earth in the red phase. The other two phases show the inverse polarity of the surge-voltage crests, and later the rise in the working voltage with respect to earth. The total duration of the surge on the red phase is 10 microseconds and its value reaches over 300 kV(max.). The rate of rise of the surge is, approximately, 1 000 kV per microsecond. There are two different time-scales on the oscillograms: that from left to right refers to the high-frequency transient, whereas that from right to left measures the 50 or $16\frac{2}{3}$ frequency of the two networks at the junction of which the oscillograph is connected. On the voltage scale, the deviation from the instantaneous value of the working voltage is measured from the full zero line; the dotted parallel is the zero line of the working voltage of frequency 50 or $16\frac{2}{3}$ cycles per sec. U_p represents the normal phase voltage of the network.

Small bundles of soft iron wires embedded in wax, mounted (*a*) in the tower leg and (*b*) in the earth wire, have proved very useful for measuring the maximum value of the discharge current [see paper (3) below for illustrations and description].

In conclusion, I should like to quote the following references to the subject matter of my remarks:—

(1) K. Berger: "A Cable Surge-generator for Heavy Loads and a Surge Potential of a Million Volts," *S.E.V. Bulletin*, 1933, No. 15, p. 325.

(2) K. Berger: "Thunderstorm Measurements in Switzerland in the Years 1932 and 1933," *ibid.*, 1934, No. 9, p. 213.

(3) K. Berger: "Results of Thunderstorm Measurements in the Years 1934 and 1935," *ibid.*, 1936, No. 6, p. 145.

Mr. E. E. Dunn: With reference to the atmospheric conditions producing summer thunderstorms, the direction of approach of such storms to the north-east part of this country is often from the south-west. The usual order of their major features is first the warm front, with its relatively high temperature and humidity, immediately followed by the towering flat-based thundercloud. Next comes the lightning, quickly followed by heavy rain. Finally, there is the passing of the cold front with a change in the wind direction and fall in temperature. Such a sequence suggests that the conditions set out under (ii) on page 2 are the usual cause of our storms.

The statement is made on page 6 that "Immediately the stepped leader stroke reaches the earth the main or return stroke begins to travel up the channel from earth to cloud." Considering this phenomenon from the point of view of the electronic theory of current flow, would the author agree that the cloud base, being negative, possesses excess electrons the flow of which "down" the completed leader constitutes the main discharge? The suggestion here is that both the propagation and the motion of the complete flash take place only in one direction, i.e. down, and the necessity for the use of the words "return stroke" arises solely from engineering methods of current measurement.

Regarding the magnetic detectors mentioned in the paper, it would be of interest to know whether the towers on which they were fitted were connected by longitudinal earth wires, and, if so, whether the current values of

30 000 to 40 000 amperes are those of individual towers or of tower groups.

The author does not appear to favour the theory that variations in air conductivity influence the path chosen by lightning, yet he is inclined to think a tendency exists for flashes to fall both on places giving a copious point discharge and on places where there are geological discontinuities, such as faults and underground springs. In connection with this, is not the presence of side branching, with components approximately at right angles to the stress, evidence of varying air conductivity? Could not the horizontal component in the very oblique path sometimes taken by the leader be the result of increased air conductivity "guiding" an ionizing point forced to propagate by electrostatic stress?

Apart from air ionization by stress, is it not probable that air conductivity, increased by radiations and emanations from electrochemical and electrothermal processes occurring at soil discontinuities and underground springs, is further stimulated by the effect of electrostatic stress on these processes, with the result that lightning finds propagation easiest through the air over such places?

Regarding the subject of overhead-line protection, except for the risk of insulator flashover due to release of the bound charge on conductors by a flash so near as 5 m., the suggestion of diverters is attractive, but it appears to me that the variant of the idea, namely lightning rods projecting 15 ft. above each pole, whilst perhaps reducing wreckage of the line when struck, would not be successful in preventing insulator flashover and consequent feeder trip-out. The magnitude of the current in the discharge, together with the usual earth resistance (5 ohms is a low average value), would nearly always produce on the ironwork a voltage differing from that on the conductors by a value exceeding the impulse flashover value of insulators used on lines working up to 20 kV.

On page 14, when referring to a stroke to a line conductor, the author takes the point of view that the current will divide, one half going off in each direction. From a practical aspect one would expect any resulting damage to be spread roughly equidistant from the point struck. Practical evidence can, however, on occasion be found suggesting decided inequality of current subdivision. As an example, following a lightning storm, 24 pin-type insulators were found damaged on a line, approximately 3 miles long, working at 20 kV, and fitted with 6 conductors together with an earth wire suspended under the conductors. The outstanding shattering and puncturing was found on 12 insulators fitted to the fourth, fifth, and sixth poles from a substation. Only one insulator flashed-over on the third pole. The remaining 11 damaged insulators—damage chiefly due to flashovers—were found on various poles up to the twelfth from the substation, principally in one direction. Inequality in the state of insulator fatigue may be considered a contributory cause, but a more logical theory for this effect would appear to be that residual field under an undischarged part of a thundercloud exerts an influence controlling the direction taken along an overhead wire by the major portion of a lightning discharge. Residual field, sometimes of increased strength, at other times of reversed polarity, has been noted by research workers and has recently been referred to in a Meteorological Office Publication, issued in 1936, on "Point Discharge in the Electrostatic Field of the Earth."

Mr. T. R. Warren: In spite of the many and varied researches which have been and are still being undertaken, records of lightning flashovers still continue to bear testimony to what is now practically the only disadvantage of overhead transmission lines as compared with the more costly underground cables. Close cooperation between research workers and transmission engineers is essential if the problem of preventing lightning flashover is to be solved.

It is admittedly difficult either to verify or to dispute much of what the author has written, because lightning records on overhead lines are not easy to obtain, but at the same time the present theories do not seem to be complete in that some assumptions which would not bear a close examination are made in arriving at the results. For example, the results of skin effect in the earth are not taken into account, and, while I can agree with the author that the resistance of an earth decreases with increasing current, the resistance will definitely increase with increasing frequency; the factor of uncertainty to which this gives rise makes it hard to believe that the final voltage across a tower which has been struck can be expressed as the product of the current and the earth resistance as measured in the usual manner. In fact, it would be difficult to explain in this way the flashing-over of a grid tower having a footing resistance of 5 ohms or less without assuming a current in excess of 100 000 amperes, a figure which, as the author states on page 7, is very seldom exceeded in practice.

It would be interesting to know whether the magnetic detectors mentioned in the paper have confirmed the existence of side flash. In this area certain of the grid lines are fitted with detectors consisting of sensitive flash-lamp bulbs, each connected between the tower and a separate earth spike driven into the ground. Their main purpose is to enable flashovers to be located without it being necessary to climb each tower, but it so happened that initially the bulbs were rather too sensitive for the purpose, with the result that during lightning flashover not only did a number of these lamps fuse on either side of the tower affected, but, in addition, well-defined groups of indicators were affected further along the line, thus affording in an unusual way unmistakable evidence of side flash on overhead lines. It is interesting to record that on a 33-kV line the side-flash band, as determined in this way, was in one instance 19 spans in width.

Prof. W. M. Thornton: With regard to the polarity of the discharge, I have always felt that there can be no essential difference between the mechanism of lightning discharge and that of ordinary unidirectional or intermittent spark discharge between electrodes. Photographic exposures taken at intervals during the formation of a spark show that it always starts from the negative pole, and the probability is that lightning does the same. The author makes no reference to the suggestion by Sir Oliver Lodge that the great destructive action of a lightning flash may be due to the discharge taking place in two stages—an A and a B spark. The former is a trigger spark between two clouds, which releases the energy of one of them to earth. Laboratory experiments support this view, but direct evidence for it would be difficult to obtain. In any case the quantity passing is very small—less than a coulomb.

There is often evidence of high electrification in mountainous districts. The "plume" on the Matterhorn might possibly be due to some such action—condensation on a kind of electric wind from a point.

Ball lightning is a most interesting phenomenon. If it is, as seems probable, a ball mostly of ozone which reverts to oxygen suddenly with explosive force, the possible horse-power on its dissolution may be reckoned in tens of thousands.

Mr. P. J. Ryle: I quite agree with the author that experience definitely indicates that low tower footing resistance diminishes the probability of flashover at insulators due to a direct stroke to the tower or to the earth conductor.

The argument on page 13 in effect assumes that flashover due to voltage between tower and conductors will not occur on the front of the wave, e.g. that indicated by the full line in Fig. 7; or, in other words, that the only tower-top potential one need worry about is the "steady state" potential closely approached in this figure after about 1·5 microseconds. This "steady state" tower-top potential is then equal to IR. This argument leads to the rather too generally accepted statement that the voltage across the insulators of any directly-struck tower can simply be taken to be the product of the tower current and the tower footing resistance. When unqualified by further explanation, this statement is optimistic and misleading. This is at once made obvious by putting the footing resistance equal to zero, when the tower-top voltage also appears as zero.

I should like to emphasize that, taking, for instance, the simple case of a tower without overhead earth conductor, the tower-top voltage rises as shown in Fig. 7 until it is chopped by the reflected wave from the tower base. The voltage amplitude reached when this chop occurs depends on the tower height, and the nature of the variation of voltage after the chop depends on the tower height, the tower surge impedance, the footing resistance, and the ratio between the two latter. The taller the tower the later is the chop, and the higher the footing resistance the less is the effectiveness of the chop. An important point is that, even with zero footing resistance, the tower-top voltage wave is not, in any case, chopped until a time has elapsed equal to $2h/v$ microseconds, where h is the tower height in feet and v is approximately the velocity of light in feet per microsecond. With very high towers, the chop may not occur until well after a time has elapsed sufficient to allow the insulators to flash over, which will consequently occur even if the footing resistance is zero. A practical illustration of this occurred some time ago. A 487-ft. double-circuit river-crossing tower fitted with magnetic-link tower surge-current recorders was struck, and insulators on both circuits flashed over. It is therefore almost certain that the occurrence was a direct stroke to the tower or earth conductor. The magnetic-link recorders gave the total tower surge current as 30 000 amperes, and the footing resistance was measured and found to be well under 1 ohm. On the simple rule of tower current multiplied by footing resistance, the surge voltage over the insulators should, therefore, have been well under 30 kV, whereas flashover actually took place across insulator strings having a minimum impulse flashover value of the order of 1 400 kV (positive 1·5/40).

Mr. H. Leyburn: Although I agree with the author that "lightning is not a blight peculiar to overhead lines," there is no doubt that, were it not for the effect of lightning upon overhead lines, the subject would be of little interest to the practical engineer. It is with this side of the problem, dealt with in the second part of the paper, that I am mainly concerned.

Until a few years ago various authorities on lightning phenomena appeared to hold the view that the majority of the lightning troubles on lower-voltage lines, say below 66 kV, were caused by induced flashovers and not by direct strokes. This view is held by some engineers even at the present time. Calculations based on this theory were made to determine the distance from a given overhead line within which lightning strokes might cause a flashover. From the absence of any reference to induced strokes I conclude that the author does not support this theory, and I should like to know his reasons.

The evidence in favour of the theory of multiple strokes is interesting and there is little doubt that such strokes can, and do, occur. It appears to me that multiple strokes lasting a comparatively long time (tenths of seconds rather than microseconds) might cause damage to protective devices such as lightning arresters and expulsion gaps, owing to the power follow-through current lasting much longer than the protective device is rated for and consequently tending to shatter it. I should like to ask the author whether he has had any experience of such shattering of lightning arresters or expulsion gaps.

On page 18 he makes the statement "A line of 900 ft. span insulated with 9 cap-and-pin units should be almost immune from lightning if all footing resistances are below 10 ohms," provided the overhead earth wire is so arranged that it intercepts direct strokes. The constants given apply to the 132-kV British grid, the tower footing resistances of which are usually well below 10 ohms, but in spite of this the grid lines are by no means immune from lightning. Is it the author's contention that all flashovers are due to direct strokes to the phase conductors themselves, and that the earth wire, although placed at the top of the tower, does not intercept them?

I now come to my main point. It is fairly clear from the paper that to make overhead lines lightning-proof (if it can be done at all) requires a much larger expenditure than is economically justifiable. The alternative to this, referred to by the author in Section 2(h), which I consider to be preferable from both an engineering and an economical point of view, is so to construct the line that it is reasonably free from lightning troubles and then to deal with the flashovers that are inevitable in such a way that continuity of supply is not affected. The operating statistics for the Petersen coil, which is one device having for its aim the achievement of this object, are interesting, but unless they are carefully interpreted they may be misleading. As is well known, the Petersen coil can clear transient earth-faults only, and cannot cope with phase faults; and, although the statement quoted by the author that 90 per cent of line-to-earth faults were cleared without interruption of supply is very impressive, it must not be overlooked that 70 per cent only of *all* faults were so cleared. It is fairly obvious from these data, and still more so from the complete results published in the references given in the paper, that a large number of phase faults had occurred on the system. Since on overhead lines operating at 140 kV, and provided with reasonably sound protection, the number of phase faults is usually negligible, it can only be concluded that the Petersen coil itself was the cause of there being a large number of phase faults, because it allows faults that start as earth faults to develop eventually into phase faults.

I believe that the number of transient earth faults on an overhead-line network, expressed as a percentage of the total number of faults, is of the order of 90 per cent, and these transient faults are capable of being cleared without disconnecting the affected line. Since, however, according to published statistics, the Petersen coil clears only about 70 per cent of all faults, and since it also increases the number of phase faults or what might be termed "cross-country" double earth-faults, it is clear that there is still room for improvement. In addition, the Petersen coil has the disadvantage that it has to be adjusted within fairly close limits to suit the capacity of the protected network. Nevertheless, in spite of the shortcomings of present-day arc-suppression schemes such as the Petersen coil or expulsion gap, in principle I agree with the author that some such scheme, which will prevent lightning flashovers from interrupting the supply, is more likely to be the solution of the lightning problem than the methods aiming at lightning-proof construction of the overhead-line network.

Mr. G. R. Peterson: I am rather surprised that, in that part of the paper referring to the effect of lightning on transmission lines, the author confines himself to the effect of direct strokes and does not appear to regard the effect of charges induced on a line as of any practical importance. In actual practice, in many cases line switches trip on earth-leakage protection during lightning storms and when reclosed remain in service indefinitely, while subsequent examination of the line reveals no trace of insulator flashover. In other cases when the location of the insulator flashover has been discovered the damage caused has been much less than one would expect had it been caused by a direct stroke. It appears, therefore, that a considerable number of the insulator flashovers caused by lightning storms are of too slight a nature to have been caused by direct strokes and have probably been caused by induction effects.

The author, in referring to the shattering effect of lightning strokes, advances the theory that this is due to the constriction of the heavy-current arc in a path whose cross-section is less than the natural cross-section of the arc in air. I should like to put forward the alternative theory that the disruptive force is due to electrostatic or electromagnetic causes. This is borne out by the fact that cases have been reported where people sheltering under a tree struck by lightning have been hurled backwards a distance of several yards away from the tree. Also, in cases of human beings struck directly by lightning, the clothes have sometimes been repelled from the body with sufficient violence to tear the cloth.

There is one peculiarity about lightning storms which I have noticed, and to which the author makes no reference, and that is that there appears to be one particular type of storm to which transmission lines are particularly vulnerable. This type appears to occur only occasionally and is generally, from the meteorological point of view, of a mild character, and yet it results in far more line outages than storms which are considerably more severe meteorologically.

Mr. T. D. Oswald: I wish to discuss the theory of cloud electrification and discharge. The author states that both Simpson's and Wilson's theories are attractive, Simpson's for its simplicity and Wilson's for its completeness. In a paper* read before the North-Eastern Students' Section in 1935 I attempted to show that Simpson's theory, with logical extension, was still capable of accounting for all the phenomena known to occur in cloud discharge and electrification as distinct from laboratory discharge between conducting electrodes.

Following Simpson, the breaking of a drop is assumed to produce electrification; and a compact region in the cloud, immediately above an upward current of air, becomes positively charged and the main body of the cloud negatively charged. Simpson explains the discharge as beginning when the field strength in a region exceeds the breakdown value of air, ionization occurs, and the mobile electrons move towards the positive cloud. The relatively immobile positively-charged air particles remain as a positive finger pointing towards the negative region of the cloud or earth. Cloud particles do not enter into the mechanism of the flash.

The field at the tip of the finger is intense, further ionization occurs, and the discharge proceeds towards the negative region. Th electrons produced are fed back via the channel towards the positive cloud, and will constitute an opposing field to the field of the positive region of the cloud. If a sufficient quantity of negative charge accumulates near the beginning of the stroke, ionization will be interrupted until it is dispersed, and this process gives the effect of a stepped leader stroke.

These leader strokes will form a network of conducting channels interlacing the negative region of the cloud. One or more of the leaders may touch the earth; in this case the stroke acts as a conductor connecting the earth with the conducting network in the negative region. The earth is positive with respect to this region, and an intense ionization proceeds up the channel from the earth to the cloud, as it is necessary to enlarge the path in order to permit the electrons in the cloud network to discharge, and this must be done from the positive end. The leader stroke produces no record on an oscillogram. The second ionization stage will show as a comparatively gradual and progressive increase in current. Finally the cloud network discharges and a rapid rise of current occurs.

In subsequent strokes the earth path is partly formed and the quantity of electrons fed back in the initial ionization stage is not sufficient to interrupt the stroke; thus no stepping takes place.

This theory accounts for the positive rain near cloud centres and negative elsewhere, observed by Simpson, the scarcity of upward forking flashes, internal cloud flashes, stepped and other leader strokes, return flashes, and the negative sign of most recorded discharges.

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* *Journal I.E.E.*, 1936, vol. 79, p. 681.

While the statement that cloud discharge occurs directly out of the negative region explains the facts, it is difficult to construct such a satisfactory mechanism of propagation as Simpson has put forward. Before discarding his explanation, the possibilities of an indirect discharge of the negative region as outlined above should be considered.

[The author's reply to this discussion will be published later.]

NORTHERN IRELAND SUB-CENTRE, AT BELFAST, 16TH DECEMBER, 1936

Mr. D. C. Blair: In view of the vital importance of the resistance of tower or pole footings a few remarks on the earth resistivity in Northern Ireland may be of interest. The Post Office have recently carried out tests of a comprehensive nature throughout the area 30 miles south and south-east of Belfast, and have been able to show that the average value of the earth resistivity in this area is about 30 000 ohms per cm. cube. Less extensive tests which have been conducted north and north-west of Belfast indicate that here the resistivity is somewhat higher, probably 50 000 to 100 000 ohms per cm. cube. The Post Office have also had opportunity to measure the resistance of about 100 pole earths; in a few cases the resistances were found to be below 10 ohms, but in the great majority they were between 30 and 300 ohms, so that flashover of insulators on such poles would appear to be inevitable.

With regard to the protective effect of earth wires, it is noticed that no reference appears in the paper as to the effect of the conductivity and internal inductance of these wires upon their surge impedance. Power lines are often provided with steel earth wires, and it seems that owing to the high internal inductance of such wires the surge impedance, $\sqrt{(L/C)}$, may be considerably higher than the typical value of 500 ohms quoted in the paper. By applying equations (22) and (28) we see that any increase in Z would apparently reduce the efficiency of the protection. The author's opinion on this point, with particular reference to steel earth wires, would therefore be appreciated. It may be mentioned that the Post Office are always interested in any factor which would encourage the adoption of high-conductivity earth wires on account of their greater screening effect where interference, due to coupling between power and telephone lines, is concerned.

Mr. T. G. Christie: We in Northern Ireland have a problem which concerns the use of de-ion type expulsion-gap lightning arresters. The Electricity Board are about to take supplies direct from the 33-kV busbars at the Harbour power station. The supply will be taken from the 33-kV armourclad switchgear through approximately half a mile of underground cable and thence on a steel-tower overhead line approximately 3½ miles to the Board's main substation. Beyond this point there are 400 to 500 miles of 33-kV overhead lines erected on wooden poles. We propose to fit de-ion type arresters at our main substation and also at the end of the 33-kV underground cable. I should like to know whether the author considers that the arresters on the first steel tower will adequately protect the cable, and if these arresters do not function whether the cable will give sufficient protection for the busbars and switchgear at the Harbour power station. One line is insulated for 33 kV, the other for 132 kV.

Mr. M. C. Cooper: Referring to the leader-main stroke sequence, it is understood that a series of individual streamers shoot out from the negatively charged cloud base towards the earth, each successive streamer advancing farther and thus blazing an ionized channel. Presumably these streamers are the result of electron discharges from the negative cloud base to the positive space charge. But the main or return stroke is said to start at the ground and strike upwards at a velocity of 1/10th that of light, and this is difficult to understand. Is the main stroke a series of electron discharges, from the now negatively-charged antenna system to the positively charged earth; these discharges commencing close to the ground, increasing in magnitude and following each other with such rapidity that they give the appearance of a single stroke striking upwards from earth to cloud? Or is the return stroke a direct discharge of positrons from the positively charged earth to the negative cloud base?

With reference to the Petersen-coil method of protection, Dr. Allibone, in his paper on "The Effects of Impulse Voltages on Transformer Windings,"* pointed out that if the voltage of the wave-front exceeded a critical value it became concentrated momentarily across the end turns, owing to its inability to distribute itself instantly throughout the winding.

It would be interesting to know whether there is any explanation for the characteristic copper-coloured cloud which nearly always precedes a thunderstorm and is seldom seen without being followed by such a disturbance.

Mr. F. Johnston: There is a very common practice in Belfast among motorists of turning on their lights in a thunderstorm. I have always regarded this as a superstition based on the assumption that turning on the lights earths the car and therefore gives additional protection. As, however, the car is insulated by the tyres this practice would seem to be a mere fetish. Personally, I always feel very comfortable in a car during a storm as I am then completely surrounded by metal: in fact a car should be rather safer than an old-fashioned bedstead, which has always been considered an almost ideal protection. I should be interested to know whether a modern saloon car has ever been known to be struck by lightning, and, if so, what damage was done to the car and to the occupant(s). Also, is Belfast the only place where the practice to which I have referred is prevalent?

Mr. W. T. Palmer: There are three points in connection with the paper which have particularly interested me.

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* *Journal I.E.E.*, 1937, vol. 80, p. 117.

First, why is the phenomenon of thunder conspicuous by its absence from the paper? Is it because of the absence of any serious research in this direction in recent years? It would seem to me, for instance, reasonable to suppose that although thunder is physically harmless it can disrupt the elementary vertical cylinder of air mentioned on page **5** so completely as to wreck the formula given there.

Secondly, it is interesting to estimate the enormous amount of power wasted in a single lightning flash. Calculation from figures given in the paper shows that it is apparently not unusual for a single flash to carry power of the order of 25 million kilowatts, which is greater than the total power generated by all the power stations in Great Britain.

My third point is in connection with two sentences on page 10: " Cattle leaning against such a fence may therefore be killed. In the same way telephone lines can introduce a dangerous voltage into dwellings." Now the Post Office provides a certain amount of protection against lightning for every subscriber, and I should like to ask the author if he would be good enough to incorporate in the paper a brief reference to this, for the sake of those telephone users who may read this paragraph and consequently experience a needless terror during the next thunderstorm simply because they do not know of the care which the Postmaster-General has lavished on them in this direction.

Mr. T. T. Partridge: In 1933 there was a very serious lightning storm in Ulster and as a result the plant of the Post Office Engineering Department was badly damaged. I should like to ask the author whether the results which were obtained as a result of that storm can be explained in view of his investigations and the theories which he has put forward.

Some ten years ago the Department decided not to fit earth wires to poles carrying communication circuits. In that storm in 1933 there were ten cases in Ulster, scattered throughout the Province, where poles of various heights, without earth wires, were shattered. The poles were shattered and splintered in the middle, but there were no signs of charring, and as the splinters had separated it was clear that the trouble had not been caused by an ordinary current discharge. Some of the poles affected were under trees, and some in the open. Among the poles which had been erected for a considerable time, every one of which was earth-wired, there was no single case of shattering. The poles under trees were dry at the time of the lightning discharge. Another peculiar feature of the storm was that where the open wires were fused the poles were not splintered and, conversely, where the poles were splintered the wires were not fused and no insulators were broken. I should be obliged if the author could explain these peculiar facts by the new theories which are divergent from those of Simpson.

Mr. J. Reilly: The author states that " the cost of proper lightning protection for a building is very considerable—in any case insurance is a cheaper proposition." I am of opinion that in ordinary city buildings both large and small there is no necessity for lightning conductors. I have been in touch with several insurance companies in order to find out what damage has been done by lightning to buildings. One company told me that during the past 10 years they had had to pay damages to a farmer for a cow, one small house in the country, and a haystack. Another company had only had 3 claims during the past 5 years—each in connection with small houses. All this makes one begin to wonder whether it is worth while to spend a lot of money on lightning conductors.

I should like to know what is the cost of proper lightning protection as compared with the ordinary method. The ordinary lightning protection that the architects allow for represents about 1 per cent of the cost of a building. I know of one instance where the cost of the lightning protection for a hospital was less than £20, whereas the cost of the hospital was £25 000 to £30 000.

I should like the author to give a brief outline of the modern method of protection.

Mr. J. H. Beaumont Robins: In regard to ball and bead lightning, the author refers to the speculation that this is composed of ozone (having the chemical symbol O_3) and that its explosive properties are due to subsequent reduction to oxygen (having the chemical symbol O_2) with consequent increase in volume, expressed by the chemical equation which shows that three volumes of oxygen are produced by reduction of two volumes of ozone. I should like to ask his opinion as to the likelihood of an alternative explanation; namely that ball lightning is composed of a mass of nitric oxide (chemical symbol NO) at high temperature, which has been formed from the constituents present in the atmosphere as a result of extreme electric stress. A suggestion of this nature was put forward some years ago and was supported by practical observations of the characteristic odour of nitric oxide, or, more probably, nitrogen peroxide, gas in the vicinity of a ball flash. Incidentally, the latter gas retains a degree of stability up to a temperature of approximately 3 000° C.

The second part of the paper deals with the more practical aspect of lightning as a menace to overhead transmission lines. It would appear that a most useful precaution is to ensure footing resistances of low ohmic value, and I would ask whether this cannot be assisted by increasing the contact area between the metallic foundations of the tower and the earth in which they are buried.

Mr. J. S. Scott: The author starts off by stating that a thunderstorm is an isolated localized thermodynamic process. I think that there can be no question as to the cause of lightning, for there seems no other reasonable way of accounting for the enormous potential energy stored in a thundercloud. Assuming that the cloud contains enough water to produce 1 in. of rainfall over 1 square mile—not unusual in heavy thunderstorms—1 in. depth would correspond to 62 500 tons. If this quantity fell 5 000 ft. relative to the surrounding air, the potential energy would be equal to 312 500 ft.-tons, i.e. 354 000 h.p. for 1 hour. The heat energy in a large lightning stroke must be equal to that of possibly several tons of nitroglycerine, and lightning acts in about the same length of time.

The author sets forth clearly the two opposing theories—those of Dr. Simpson and Prof. Wilson. I believe there is a strong possibility that the base theory is the correct one. Another support for the thermodynamic process is that when a violent volcanic eruption occurs there are almost invariably very heavy lightning discharges within the hot gas stream from the volcano.

In the latter part of his paper the author points out the effects of lightning strokes on buildings where the structural steelwork is discontinuous or not well bonded. About 11 years ago I was working in a workshop about 100 yards from a 5-story brick building. There was a heavy thunderstorm—a brilliant flash of lightning immediately followed by a peal of thunder. I found that the building had been struck and that the top floor, which was a hay loft, had caught fire and damaged the machinery. The building had reinforced-concrete floors. There was in the brickwork a slight trace of cracking, but I do not know whether this had been caused by the lightning. I am of the opinion that an induced spark discharge set the hay on fire, and it seems to me that reinforced-concrete buildings should, if possible, have all the metalwork thoroughly bonded and earthed.

Another case, which can be seen to the present day, occurred at Scrabo Tower. When the tower was struck the stonework cracked at the base, where apparently the lightning conductor had not been properly earthed.

With reference to ball and chain lightning, this, I believe, used to be considered a subjective phenomenon, but there is now very strong evidence that it really occurs. In certain experiments carried out at Queen's University, Belfast, a small bead of light has been seen travelling down one of the vacuum tubes, and if this effect could be repeated it might lead to further information regarding ball lightning, about which very little is known.

Mr. F. H. Whysall: The author's explanation of how and why lightning strikes is very interesting, and it is evident from the figure of voltage-rise quoted (10 million volts) that a lightning stroke on a transmission line will be attended by very serious results. Happily Northern Ireland, like the North of Scotland, is practically immune from thunderstorms, and the problem here is not nearly so acute as it is in Africa, Central America, and Eastern India. Indeed, the number of thunderstorms recorded in the North of Ireland is only about a quarter of that occurring in the South of England. Nevertheless, there is danger in its very rarity, since when lightning does strike it may find the engineer unprepared and the damage caused may be correspondingly increased.

I am disappointed that the author does not provide a schedule of the different forms of protection in use in various countries, showing the recorded efficacy of each type. Each form of lightning protection has its own adherents amongst the experts, and therefore it is only from such a schedule that the supply engineer can obtain comprehensive and reliable information.

Regarding the earthing of individual towers on a 33-kV system, it would be necessary, in order to prevent a voltage-rise exceeding 300 000 volts in the event of a lightning stroke, to have a tower footing resistance not exceeding 2 ohms. Even with this value of resistance, which is very difficult to obtain and maintain, it seems to me that flashover of the line insulators would occur.

In 1930 it was suggested by Dr. Petersen that, to achieve a completely lightning-proof overhead line, lightning rods should be installed on the towers, the towers should be connected by an earth wire of good conductivity, and each tower should be effectively earthed. This would afford efficient screening for all live conductors and would ensure that lightning strokes occurred only to earthed parts. Can the author offer an opinion on Dr. Petersen's suggestion?

[The author's reply to this discussion will be published later.]

NORTH-WESTERN CENTRE, AT MANCHESTER, 5TH JANUARY, 1937

Dr. J. L. Miller: The paper is noteworthy not only in that it deals consistently with the complete story of lightning but also in that the attack is made in a manner such that there is an orderly progression from the original electrification of the storm cloud to the final surge voltage appearing on the line immediately after a stroke. This method, which is obviously the most logical one but unfortunately in this subject also the most unusual one, is difficult because there are both numerous alternative theories to be weighed and also several gaps between succeeding parts of the story to be covered. The alternatives are, however, all considered with judgment and the gaps are invariably bridged with skill; in particular the substance of Appendix 1 deserves very careful consideration.

There is still, however, a considerable lack of correlation in connection with one important question. I refer to the hiatus between the physicist's theories of the initiation of the flash and the engineer's assumption that the stroke may be considered to be a conductor having a surge impedance and upon which exist the travelling waves of voltage and current that give rise to the surges in transmission lines. Anything that can be done to link up these two aspects of the phenomenon and to clear the air with regard to the admissibility of a stroke surge impedance—my opinion is that the latter assumption is reasonable, and I have previously used it—will be of value, and I hope that along with the possibly more interesting questions of cloud electrification and surge-voltage limitation these two problems will receive their due measure of thought and experiment.

In the meantime it is interesting to recall that Fortescue has given an explanation that assists us in at least visualizing the phenomenon. Put briefly, he assumes that the standing wave represented by the negative space charge surrounding the channel produced by the initial streamer divides, when the tip of this streamer (which is supposed to behave like a charged conductor) reaches earth or a line, into two equal negative travelling waves, one moving down into the line to be reflected and refracted according to the classical laws, and the other moving upward to release more cloud energy to feed the channel.

In dealing with the quantities involved in the lightning stroke the author can do little other than employ average values. Nevertheless, in regard to the magnitude of the stroke current which, with the tower footing resistance, is the most important factor influencing insulator flashover at a stricken tower, he perhaps errs on the optimistic side. He states on page 7 that "the most frequent value (mode) is between 30 000 and 40 000 amperes. Currents greater than 100 000 amperes occur very seldom." A great deal depends on the interpretation that a reader may give to this statement, and it may therefore assist if I quote from a recent paper* which shows that 25 per cent of strokes estimated from results obtained on two particular lines exceeded 55 000 amperes, and that 10 per cent exceeded 100 000 amperes. These figures indicate that the expectancy of strokes in, say, the 40 000 amperes to 70 000 amperes range is reasonably high, and more than add confirmation to the importance brought out in the paper of maintaining low values of the tower earth resistances to obtain reasonable immunity from flashover troubles.

In this connection I am rather sorry that the author does not deal with the counterpoise more fully, since in high-resistance terrains this device appears to have proved successful. As is stated in the paper, this additional buried conductor at the tower foot behaves for a few microseconds as a surge impedance and materially reduces the tower-top voltage and hence the risk of flashover. It can be shown, using the simplest assumptions, that one equation for the tower-top voltage is $2E_0Z_cZR/[Z_cZ(Z_0 + R) + 2RZ_0(Z + Z_c)]$, where Z_c is the counterpoise surge impedance and where the other symbols represent the same quantities as in the paper; this equation indicates that with an earth resistance of the order of 200 ohms (the other constants having usual values) the use of the counterpoise reduces the tower-top voltage to about one-third.

The paper brings out very well the need for large clearances between the earth wire and the line conductors and should do something towards making some engineers realize the utter futility of the very small clearances we so often see on some of our 6-kV and 11-kV lines. I do not think, however, that the statement (page 18) "The infrequency of mid-span flashover suggests that stroke wave-fronts as short as 1 microsecond must be uncommon" really proves anything. Actually it could be argued that even with a relatively large percentage (say 10 per cent) of steep-fronted waves many mid-span flashovers are not to be expected. For instance, taking 200 ft. on each side of the centre of a 1 000-ft. span as being sufficiently removed from the towers to allow of treatment similar to that given in Section 2 (*f*), then only 40 per cent of strokes come into this category. If it is assumed that 10 per cent of strokes are very steep then the number of mid-span flashovers will be 4 per cent of the total strokes connecting with the line. With a lightning expectancy of, say, 30 strokes per 100 miles of line per annum, this means that on the average there will only be one mid-span flashover during this period.

It is very difficult to estimate the behaviour of a surge after insulator flashover; and in this connection it is interesting to recall that in a large number of cases more than one tower is involved in a flashover. Several sets of data are available and they show that about 40 per cent of strokes involve flashovers on two or more adjacent towers. It is this, of course, which partly accounts for the diverse shapes of the travelling waves found on conductors.

Mr. W. Fennell: In the past the majority of lightning protectors have been designed without knowledge of what these devices have to protect the lines against. Now that the author has gone into the subject properly we may hope that the designs which are sold to us at a very high price for protecting our lines against lightning will, at all events, provide some protection.

I hope he will explain to me what the word "tropepause" means. According to the dictionary a "trope" is "a figure of speech."

On page 11 the author states "'Floating' fireballs have the red colour of meteorite tracks in the lower atmosphere. They shun good conductors and generally choose a path through the air." This remark gives credibility to a very detailed account my father gave me of a fireball he once saw when he was in school. This fireball came down the chimney, travelled slowly across the floor, and went out through the door, which had been left open because of the great heat preceding the thunderstorm.

On the Mid-Cheshire system we have 33-kV, 6·6-kV, and 3·3-kV lines and find we get considerably more lightning trouble with the lower-voltage lines. In fact almost never have we traced breakdown on the 33 000-volt lines to lightning. Taking into account the enormous voltages referred to in this paper one would think we should have had some trouble with the 33-kV lines. Experience of this sort is fairly uniform throughout the country. We have certain 6·6-kV and 3·3-kV sections where we usually get fuses blown when there is a thunderstorm in this part of the country. One area is along the Delamere Ridge between the Chester Road and Delamere stations. The Delamere subsoil is pure sand about 80 ft. deep, and no water is found until deep in the sandstone rock below it. The surface is practically insulated from the mass of earth. The fact that it is one of our bad spots seems to contradict certain of the statements in the paper. Does the author think that we really do get direct lightning strokes on our lines in this country?

I have been told by Post Office engineers that 200- or 300-yard sections of their very small lines sometimes completely disappear in bad thunderstorms. I have never known a single section of wire in one of our lines, even a No. 8 S.W.G., disappear as the result of a lightning stroke. Presumably the Post Office 10- or 20-lb. per mile lines will not carry the "earth" current, indicated in the paper, even for a very short time, and act as fuses.

I should like to know whether the author thinks a rod of a material known as thyrite is a type of protection which is likely to be of use. It is one of the most recent offered to us.

Mr. W. H. Thompson: The part of the paper that seems to me to be difficult to understand is that which states that the lightning stroke, although initiated by a number of streamers or leader strokes, results in a return or main stroke in the opposite direction. I should imagine that as a cloud became charged, say on the Wilson basis, with the lower surface negative, the atmosphere between this cloud and earth would become the repository of an electrostatic field which would grow in intensity as the charge on the cloud increased. This field would have a form and direction in strict relationship to the contour of the charged cloud and earth. The most intense part of the field would be either at the cloud end of the field or at the earthed end, depending upon the formation of these. The free electrons in the atmosphere would find themselves affected by the growth of this field and would move along their respective lines from the cloud to the earth, and with an acceleration governed by the rate of increase in the field strength. As this field is more concentrated at the earth and cloud ends, it is to be expected that greater ionization would occur there, and therefore visible streamer discharge would commence there. As the field intensity increases owing to, say, further charge concentration on the cloud, these streamers reach out until one reaches the earth electrode, and the main stroke passes.

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* W. W. Lewis and C. M. Foust: *Electrical Engineering*, 1935, vol. 54, p. 934.

The author tells us that this main stroke passes in a reverse direction to that of the leader strokes that have led up to it. This suggests that the field has suffered a reversal of direction upon the leader touching the earth. I should like to know the cause of this, since I cannot see any justification for it. It would seem to me that the negative charge on the cloud-base has, through the medium of its leader strokes, passed some of its energy to the atmosphere, and that, when a leader touches the earth, a relatively low-resistance path is provided for the complete transference of the remaining energy on the cloud-base, plus that previously given to the atmosphere in the form of the accelerated electrons. Thus the main stroke will be the fulfilment of the efforts of the leader strokes. It is quite conceivable that the main discharge may overshoot itself and result in a secondary stroke in the reverse direction.

As regards the greater luminosity of the main stroke at the earth end, would this not be explained by the fact that the whole electron output in the form of leader and main strokes concentrates on the earth electrode and is subject to intense collisions with the positive space charge about the earth end?

I have seen a number of photographs of discharges from points, and in nearly all of these the streamers emanate from the positive electrode. The explanation of this, I would say, is similar to that given above for the lightning stroke, i.e. that there are more intense ionic collisions within the region of the positive space charge, and that there is actually a minute time-lag between the discharge at the negative electrode and the absorption at the positive electrode. This may be regarded as arising from the inertia of the intervening dielectric to the action of spark discharge.

Mr. G. F. Sills: When I was interested in electrical work in Ontario for some years, the lightning storms were very severe, and lightning arresters were considered of great importance. A curious incident occurred at a high-voltage pumping station. Although at that time the weather was settled, the arresters used to function early in the morning. This was found to be due to switching operations at a nearby 110 000-kV substation.

For many years the *Transactions* of the American I.E.E. have given complete operating results in connection with lightning arresters on the systems of the various power companies in America, and in a recent issue I read that in one case only 13 towers with overhead earth wires had flashed-over, compared with 325 on an unprotected line; reducing this data to numbers per 100 circuit-miles per year, the comparison is 7 per cent as against 93 per cent. It seems to be established in America that an overhead earth wire is essential, and yet in some cases high-voltage towers are still erected without overhead earth wires. Can the author suggest the reason for this?

On page 19 the author refers to the use of a counterpoise with the idea of preventing outages. A recent issue of an American technical paper states that in one case a counterpoise introduced during the spring of 1929 had prevented flashovers. A number of towers had suffered during the 3 years prior to its installation. A surprising feature of the measurements made in this case is the considerable distance travelled by the counterpoise currents; a stroke to one tower drew current from nearly a mile away, and to other towers drew current from 1½ miles away. It is also interesting to notice that in America a stroke current of 1 000 amperes is regarded as only a "trace," the maximum stroke currents being of the order of over 100 000 amperes. Of the tower-structure currents from the 220-kV line, 66 per cent have been less than 10 000 amperes, and only 11 per cent have exceeded 30 000 amperes. The very severe currents occasionally registered elsewhere have not been experienced on this particular American system.

Mr. O. Howarth: The earth wire over the line along the tops of the towers is held by some engineers to be an inadequate protection, and in fact to be almost useless. After reading the paper I have come to the conclusion that the reason for this is that on the lower-voltage lines the distance between the earth wire and the other wires is insufficient to enable it to be an effective protection. If the earth wire is struck, it flashes across to the power conductors. It seems that there is no adequate protection against a direct lightning stroke to a line except plenty of insulation, which, of course, is impracticable on lower-voltage lines.

Is the amount of energy liberated by the lightning stroke very small? If so, and if the power current can be prevented from following, the mechanical damage done will be so small that the line can be put back into service immediately after it has come out.

I once came across a rather curious case where a lightning storm had damaged a meter and the lightning current had gone to earth on the consumer's installation. The current had evidently passed through the meter series coils and repelled the disc, jamming it so hard against the pole of the shunt element that it had shaped itself perfectly to the bottom of the shunt pole.

Mr. F. Walker (Cheshire) also took part in the discussion, quoting two historical references. The first was to Benjamin Franklin's well-known kite experiment in June, 1752, and the second a communication from

Alexander Volta which was read before the Royal Society on the 14th March, 1782.

Mr. E. T. Norris also took part in the discussion. The substance of his remarks will be found in the report of the London discussion (see page 27).

Mr. A. B. Mallinson (*communicated*): Last summer we had an experience of a floating fireball at the works adjoining my office. The ball came slowly out of a closed room which has 2-ft. walls on all three sides and is used as a drying stove. It was pear-shaped and of a dull red colour, and as it floated lazily out of the door of the drying stove, across a wooden landing and over the top of a motor lorry, there were short streamers of red flame coming out from it in all directions. Eventually, when it reached about 3 ft. from the ground, it burst with a tremendous bang. By this time the rain had started. Simultaneously with the appearance of this fireball the circuit breaker on the lighting system was tripped and there was an outbreak of fire. This occurred on the opposite side of the 2-ft. wall from the room where the fireball had come through, at a point where the lighting wires passed within a few inches of the lightning conductor coming down from the mill stack. I made a very careful examination afterwards but I could find no sign of anything having gone through the wall into the room where the fireball started, yet I feel sure there must have been some connection between the two.

Mr. F. J. Miranda (*communicated*): In support of the modern view that lightning flashes are localized at points where there is a discontinuity in the conductivity of the soil, Stekolnikov made an experiment which is described in the present paper. This experiment was repeated under various conditions in the high-voltage laboratory of Messrs. Metropolitan-Vickers, and it was found that the localization of the flash was a function of the applied voltage as well as of the characteristics of the soil. A description of the experiment may be of interest.

A metallic plate was solidly earthed and covered with evenly distributed damp sand, which was carefully levelled to form a plateau; on this were built two sand "hills" of conical shape, and a brass sphere was buried between them. A long brass tube was suspended over the hills, its axis being parallel to the plane of the plateau. This tube was connected to an impulse generator and thus formed one electrode of a spark-gap of which the other was the earthed plate. Reference to Fig. E will show the layout employed. The impulses were of negative polarity and of the standard 1/50-microsecond wave-form; that is, a wave in which the voltage rises to the crest value in 1 microsecond and falls to half this value in 50 microseconds after the beginning of the wave.

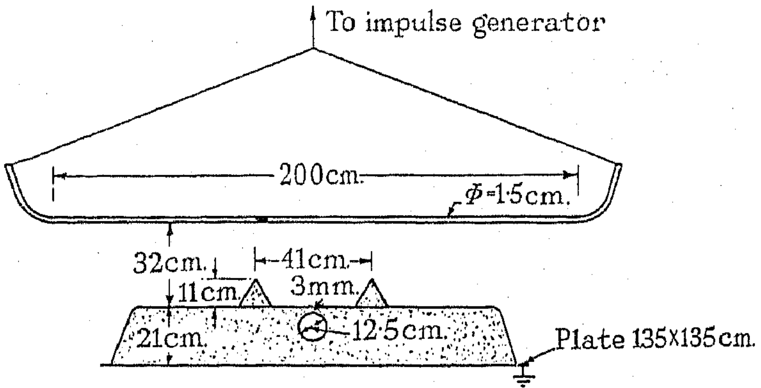

Fig. E

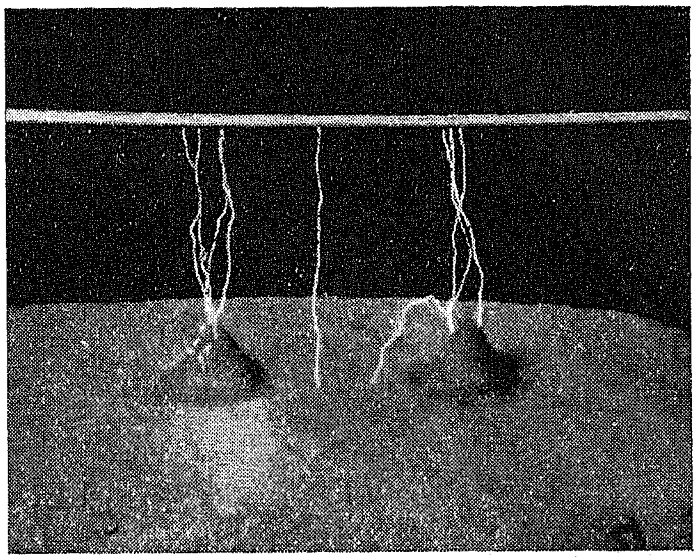

Fig. F

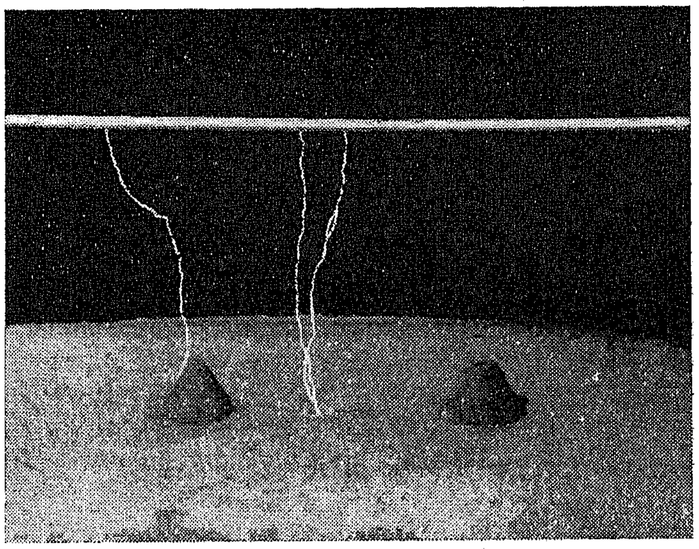

Fig. G

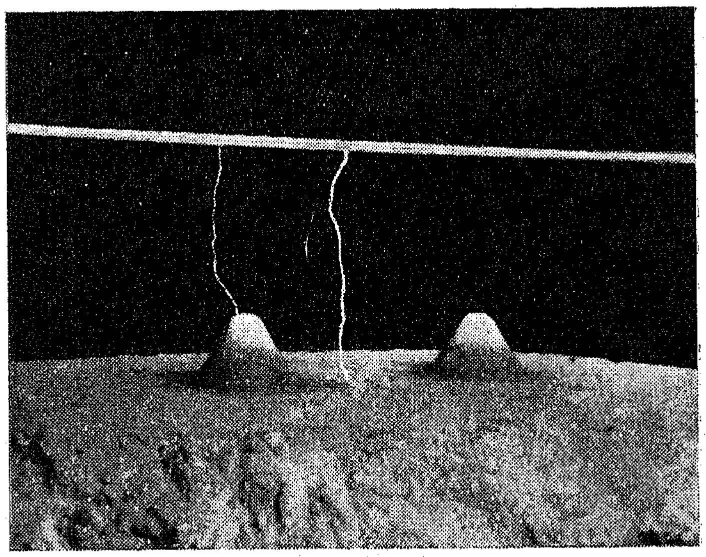

Fig. H

It was found that at voltages just sufficient to cause sparkover (minimum impulse sparkover) the spark always passed to one or both of the hills, but at voltages greatly in excess of the minimum sparkover voltage the spark always went to the sphere. At intermediate values of voltage the sparks passed either to the hills or to the sphere, the percentage of sparks passing to the sphere increasing with the voltage when the electrode spacing was kept constant.

Fig. F is a reproduction of a photograph which was taken when the crest voltage of the impulse was 210 kV, at which value 95 per cent of the sparks went to the hills and 5 per cent to the sphere. Similarly Fig. G refers to tests when the voltage of the impulse was 450 kV, for which the distribution was 30 per cent to the sphere, 70 per cent to the hills. For an applied impulse with a crest value of 700 kV Fig. H illustrates the 100 per cent sparkover to the sphere; in this case secondary sparks of low intensity were observed to pass to the hills in 4 out of 100 applied impulses. Fig. H shows this secondary spark.

It will of course be obvious that the crest voltages quoted were not actually obtained across the spark-gap, owing to its chopping action. In other words, the faster the rate of rise of voltage, the more does the spark tend to ignore high-resistance paths in favour of those of lower resistance, even though the latter be considerably longer than the former.

This experiment furnishes a possible explanation for the existence of areas more subject to direct lightning strokes than others. The experiment is open to criticism because the live electrode is not strictly equivalent to a cloud: a metallic conductor is substituted for a space-distributed charge, and the conductor is not charged prior to the arrival of the impulse.

[The author's reply to this discussion will be published later.]

SOUTH MIDLAND CENTRE, AT BIRMINGHAM, 11TH JANUARY, 1937

Major A. M. Taylor: I should like to ask the author whether it is not allowable to assume that, under instantaneous conditions, the lines of electrostatic stress would distribute themselves in relation to the earth wire in the same way as under steady conditions. If so, it seems to me that, at Boulder Dam, where the earth wire is some 20 ft. higher than the power conductor and is displaced some 7 ft. nearer to the centre line of the tower, the power conductor is in danger of having a voltage of perhaps 10 000 000 volts impressed upon it when the flash takes place to the earth wire, which would of course flash-over across the insulators. This, however, would only occur if the discharge from the cloud was perhaps at an angle of 45° from the vertical; but a serious value would occur even at 30°. This reasoning is based upon an equipotential surface corresponding, very roughly, to an arc of 15-ft. radius struck from the earth wire as centre and carried upwards from the live wire to a point, say, 15 ft. (measured from the earth wire) along the direct line of the flash. Taking 50 kV per in. as the (average) disruptive stress in the air along the line of discharge immediately before the discharge actually takes place, then the potential at the aforesaid point is 9 000 000 volts above earth; and the power conductor, being roughly on the same equipotential surface, is momentarily (until the discharge starts) raised to the same potential, and hence the insulators will flash over. If this argument is correct, our own C.E.B. lines are hopelessly unsafe in this respect. The Boulder Dam double-circuit towers are, from this point of view, very much safer than the single-circuit towers.

Another point about the Boulder Dam lines on which I should like information is the following. The two lines of towers, where they cross the desert, are separated by 100 yards—nominally to give greater protection from lightning. On this 150-mile stretch of line the extra cost of 2 more miles of line would be negligible, compared with the advantage of not running risks of interruption. Why, then, were not the lines separated by at least a mile, as is done in South Africa? Would not this arrangement have enormously reduced the chance of both lines being simultaneously put out of commission? All the expense of sectionalizing the line would, in a sense, be wasted if this occurred.

Regarding the question of the sectionalization of the Boulder Dam lines, was not the bringing together, into one switching house, of line conductors from both lines (as is done at the two switching stations) a source of weakness; and especially since the lines were only 100 yards apart? Could not an appreciable portion of the 10 000 000 volts on the failing line be transmitted across to the other line, by these cross-connections?

Mr. F. H. Clough: I dealt with the practical aspects of the origin of lightning and its effect on electrical supply in my Address* to this Centre a little more than a year ago, and do not think there is much that I can add to what I then said. The problem of obtaining continuity of supply during thunderstorms can be divided conveniently into two portions. Firstly, the designer of the transmission line must provide suitable paths for the lightning stroke to pass readily to earth, but ensure that there shall be no subsequent power flow over these paths: he must also arrange that the travelling wave caused by the lightning stroke is of limited amplitude. Secondly, the designer of terminal apparatus must arrange that the distribution of voltage in the apparatus arising from the travelling wave shall be as uniform as possible, and that there shall be adequate insulation to withstand such voltage.

Mr. J. A. Sumner: I am interested in the author's request for information as to the effect of lightning on particular schemes. When the distribution system with which I am associated was commenced approximately 5 years ago, I arranged for all faults to be carefully recorded and classified, and a map of the district was prepared showing the lines in diagrammatic form. Each fault that occurred was then marked upon the map under a special system of reference, and in this manner we obtain a graphical representation of faults. It was noticed, after about a year, that lightning faults were occurring in three particular districts, and as the time was extended it was found that lightning troubles were associated almost entirely with these three particular points in the area.

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* *Journal I.E.E.*, 1936, vol. 78, p. 49.

During one of the holidays last year, I was at the top of a high hill in Shropshire, the Wrekin, and was able to observe the course followed by a very heavy rainstorm which was proceeding. Although rain was falling heavily within approximately ½ mile of the hill upon which I stood, I observed that it moved round the hill, and followed almost exactly a course, approximately 10 miles in length, which was associated with the three districts in which lightning trouble occurred. I notice that the author refers in his paper to the fact that " In mountain country, convection is assisted by the heating of the slopes (valley breeze) and the upward deflection of warm damp winds," and if the course of the rainstorm is the course followed by a lightning storm, as I imagine could well be the case, this would account for the localization of lightning along a path which is shown quite clearly by the fault map.

I notice that the author states that earth wires have proved to be " reasonably " effective interceptors. My own experience leads me to think that this statement could be made even stronger, and to suggest that for all except direct " strikes " on to or very near to the line, an earth wire placed at the top of the tower, or pole, and above the phase conductors, is an almost certain safeguard against lightning trouble. On the system with which I am associated, the 33-kV lines are supported on steel towers, with an earth wire carried above the phase conductors and symmetrical about them. Until approximately a year ago the 11-kV lines were erected without a separate earth wire, earthing being carried out on each pole. There is only one case of lightning affecting the 33-kV lines, but a large number of cases where lightning has affected the 11-kV lines; in the one case on the 33-kV system I suspect that the trouble originated on the 11-kV line which was tapped from the high-voltage system, and, owing to the failure of the switch installed at the origin of the 11-kV line, the lightning trouble passed from the 11-kV into the 33-kV system, and burned out a 33-kV air-break switch on the steel tower. This, I think, is fairly positive evidence of the effectiveness of an earth wire placed above the phase conductors, and I am arranging for all 11-kV lines which have been erected in the past year, and which will be erected in the future, to have an earth wire carried above the conductors; I think it was a mistake not to do this in the first place.

It is very difficult to obtain proof of the effectiveness of lightning arresters. A number have been installed on the 11-kV system, but there is no positive knowledge that the arresters have operated, and only an approximate view of their effectiveness can be obtained after a reasonable period of time, by comparing the number of lightning faults for, say, 2 years, with those which had occurred in the previous period of 2 years. This method is, of course, invalidated to some extent by the lack of knowledge of the number of lightning storms which have occurred during two periods used for comparison.

The author states that an earth wire works less successfully on lines which have low insulation. I should like further information on this point, because, in his verbal summary, the author stated that the voltage induced in a conductor by lightning may be as high as 200 million volts. I cannot see that it matters very much whether a 33-kV line is insulated for 100 or 2 000 volts if it is called upon to deal temporarily with a voltage as high as 200 million volts.

Mr. H. S. Dransfield: I should particularly like, if possible, more information about ball lightning, whose mystery has always intrigued us. I understand that we still know very little about it. I have never been fortunate enough to witness it myself, but I remember a relative stating that, when working a morse instrument in a telegraph office many years ago, he actually saw this phenomenon as a distinct ball of fire on the leading-in wire. He immediately made a hurried exit from the room, and shortly afterwards an explosion occurred which wrecked the instrument. It does seem, therefore, that there is good evidence for the occurrence of ball lightning, but I confess I can hardly visualize balls of highly charged ozone appearing through keyholes or cracks in doors, which I notice the author mentions. I would add that some years ago in the course of some experiments with a 2 000-volt storage battery of Planté cells it was found that at one pole, with suitable contact, a phenomenon very similar to ball lightning on a small scale was produced.

With regard to the protection of overhead lines, the author states that with ordinary lightning strokes something is known of the space charges; but is anything known with regard to the effect of ball lightning on such lines? One would expect something very different from the effect of a direct lightning stroke.

Mr. R. H. Rawll: During the War I happened to be going up a hill when a fireball appeared to come down and hit the ground about 100 yards away with a loud report. I went over to look at the spot and the only thing I could observe was a certain amount of flattening of the grass. The author states that " Fireballs which fall from the clouds usually hit the ground and explode." Does this mean that there is little or no electrical charge in a fireball and that the explosion is simply a physical discharge of gas rather than a discharge of electricity? If this is so, a fireball hitting a transmission line might have a very different effect from that of an ordinary lightning stroke.

Mr. E. A. Reynolds: The author gives a sound theory regarding the static charges in thunderclouds, and explains how they are formed. I think, however, that he knows of experiments that have recently been made which prove that his theory of the negative base of the thundercloud is correct, although in this theory a loophole is left for those who hold that the base must be positive by the statement that there are probably centres of positive charge in the bottom of the cloud. The separation of positive and negative charges in the thundercloud through the upward current of air can apparently only take place in a freezing atmosphere, so that the water drops have to be turned into ice before the positive and negative static charges can be separated. If this is so, it would appear that this action cannot take place except in that part of the atmosphere where there is perpetual freezing, and therefore these clouds must extend to a very great height. It would be interesting to know the approximate depth of these clouds and the height from the ground of their bases during a thunderstorm.

With reference to the author's remark about a number of animals being killed by one flash of lightning, I think it was the voltage gradient along the ground that was responsible for this. In the case of leakage from an overhead line to ground with a poor conductivity there is a very big voltage gradient radially from the point of contact with the earth, and animals have been known to be killed by a very small difference of voltage in the ground between their front and hind legs due to a comparatively small leakage current. When we are dealing with currents of a few hundred thousand amperes the voltage gradient over a large radius is very great, and it is quite possible that all the animals that were standing more or less radially with respect to the ground point of the flash would have enough voltage across their legs to kill them: only those who were standing tangentially to this gradient would escape.

The author gives some very interesting details of various experiences of fireballs or thunderbolts, but gives no theory dealing with them, and unfortunately there is very little information on the subject available. It appears strange that up to this time no one has put forward any theory covering the formation of these phenomena, although they have been in existence as long as lightning itself. I should like to ask whether it is suggested that they are material bodies in some unstable form, or whether there are any other suggestions as to their composition.

Mr. E. T. F. Onley: The author's picture of a lightning flash assumes that it takes place between a cloud and earth. One sometimes sees a flash between two clouds, however, without any apparent continuation to earth. Am I to take it that one cloud is in series with the other on the way to earth and that the discharge passes through the second cloud and continues to earth at some distant point out of sight of the observer?

Mr. V. N. Halliday: In the author's slide showing a lightning stroke hitting a tower well below the top of the lightning conductor, at the right-hand side there was a peculiar flash with a re-entrant angle or two in it which rather intrigued me. Also, in his photograph of lightning flashes to sand-hills with the buried ball in between, there was one that flashed from the top of the sand-hill to the ball. I should like to know whether this occurs very regularly.

Mr. R. M. Charley: A personal reminiscence may be of interest. In 1917 when returning from a patrol at about 15 000 ft. I was obliged to pass through a thunderstorm, and the experience was rather alarming. On several occasions I flew in thundery weather, and I have vivid recollections of the very beautiful cloud formations. Usually there were detached mountains of white cloud extending from 2 000–3 000 ft. up to perhaps 16 000 ft. When the sun was shining a marvellous spectacle was seen while flying above and amongst such clouds.

[The author's reply to this discussion will be published later.]

EAST MIDLAND SUB-CENTRE, AT LOUGHBOROUGH, 19TH JANUARY, 1937

Mr. W. F. Furse: Sir George Simpson is of the opinion that the portion of a thunder cloud nearest the earth is positively charged, and Prof. Wilson proves it to be negatively charged. Does it matter, from the practical point of view, which of these theories is the correct one? One of the experts to whom the author refers mentions in his treatise what he calls "A" and "B" strokes: he regards one cloud floating above another cloud as forming a condenser, and calls a discharge between the two clouds an "A" stroke and a discharge from the under-side of the bottom cloud to earth a "B" stroke. On this theory the lightning is always regarded as passing from the cloud to earth, but the author does not hesitate to refer to a stroke from earth to the cloud. I should like to have his reasons for this assumption.

Experience in this country shows that some districts, e.g. the Vale of Belvoir, have more trouble from lightning than others.

As regards lightning-conductor design, if a lightning conductor is fixed to the highest point of a building what area relative to the height is supposed to be protected? The author states that it is not unknown for a flash to miss the lightning conductor altogether, but I assume that, providing the lightning conductor is well earthed and all external metalwork is bonded to the conductor system, the building will not be affected. I think it is far better to protect against lightning than to insure against the risk of damage. I believe absolute protection can be given if all metalwork is harnessed to the conductor system by a metal cage both inside and outside the building, such as the Government specify for the treatment of explosive magazines.

With reference to the difficulty in earthing met with in some parts of the country where there are non-conductive surfaces or dry subsoils, this can often be satisfactorily got over by sinking tubular earth connections, the top portion being fitted with a spout to which a water supply can be led (it may only be surface water). Within my experience earths have been sunk in this manner as low as 30–40 ft. below ground, until the proper stratum has been reached.

Mr. J. P. Tucker: The author stresses the value of an aerial earth wire, but I should like to point out that the existence of such a conductor does not give one complete confidence during a lightning storm. Some undertakings have experienced less frequent outages after the aerial earth wire has been removed. Is this due to footing resistances?

The paper attaches little importance to claims that certain localities or objects receive an undue proportion of lightning strokes. I have a report here which seems to show that lightning has an affection for certain trees, the oak and the elm being especially preferred. Has the author dismissed this question of preferred localities and objects too lightly?

I am impressed with the comparative immunity from lightning troubles of the very big mileage which exists of low-voltage overhead lines, and particularly with the small amount of damage done to consumers' installations. Can the author explain why a lightning storm in a certain locality will bring out the circuit breaker on the e.h.t.

system with no uncertain force, and yet the l.t. overhead system in the same locality will be apparently unheeded by the lightning? I have investigated several outages where the lightning has caused the e.h.t. pole transformer fuse to blow, but an inspection of the l.t. side and of both the outside and the core of the transformer has revealed nothing unusual. Surely such storm or power energy should show some indication of its path after flowing through the fuses?

Some years ago Prof. Boys suggested initiating lightning strokes by shooting rockets into the thunderclouds, the rocket to have attached to it a fine conducting wire. Has such an experiment been carried out, and, if so, with what result?

Mr. A. Brookes: Direct lightning strokes on communication lines for telephones and telegraphs are of rare occurrence compared with inductive effects due to thunderstorms. These may give very steep wave-fronts on redistribution at the time of the flash, perhaps a mile or so away. Owing to transposition of the lines these are well balanced and the induced voltages developed on the telephone circuits are relatively small, although they may reach high values from lines to earth. Heavy chokes or discharge coils with centre points earthed are used at the ends of the line to dissipate the induction charges without causing damage to exchange apparatus or giving loud clicks in receivers. These coils are arranged to give a heavy bridging impedance for speech and carrier currents, but low impedance to earth from lines in parallel. Such coils are used to minimize the effects on the terminations and not to attempt to drain the whole line, and are generally restricted to one at each end, with possibly one in the centre of a long line, so as to minimize the speech degradation.

Regarding the distribution of charges in a cloud, whichever theory one takes, the prime factor appears to be the concentration of charge, e.g. the positive charge may be concentrated at one region of the cloud and the negative charge spread through a large proportion of the body of the cloud, which is probably what occurs.

I should like to know the author's views on the streamer path—whether this consists of electrons or ions. In a lecture on lightning which I gave some 20 years ago, I mentioned this streamer path and stated that it was due to the flow of electrons as brush streamers, which ionized the air path and thus allowed the rush of positive ions to cause the spark. The stepped leader type of stroke was not then known. The positive and negative Lichtenberg figures included in Dr. Dunsheath's Address* as Chairman of the Transmission Section are interesting in this respect.

There is still controversy as to the correctness of Simpson's and Wilson's theories, and I think the author is correct in his statement (page 4) that phenomena in actual thunderstorms are more complex than has been suggested by either. More requires to be known about the insulating properties of a thundercloud and the spread of the potential variations.

There is one point which I should like to cover, and that is the growth of potential with coalescence of drops. We know that electrical charges are generated when drops are broken up by the ascending air stream. Those droplets coalesce higher up and, owing to their increased weight, fall, to be again broken up and further charged as they are driven upwards, where they again coalesce and fall towards the storm centre, and so on. When a flash occurs some of the charge is released, thus allowing the large drops to fall, giving the heavy downpour generally following a flash. Now if we consider eight charged droplets coalescing to form one larger drop, the total volume of water is unchanged except for evaporation, which would only exaggerate the effect, and as the volume of a sphere is proportional to r^3 the new drop will have twice the radius of the original droplet and so twice the capacitance; hence the potential will increase by $Q/r = 4$ times. I think this point has some bearing on the creation of higher potentials, as a drop may be formed from a considerable number of droplets.

Is there any necessity to consider a second leader stroke for a multiple flash? The latter could readily follow the lowered resistance of the ionized path, as it is hardly conceivable that such a path would be completely discharged.

Mr. Furse's remark, to the effect that it is unimportant whether the potential of the storm centre is positive or negative, may have some bearing on the action of a lightning conductor. If it be considered that negative electrons pass into the conductor then the latter carries the energy of the flash, whereas if it be considered that the electrons pass up the conductor the action of the latter may be to relieve the potential stress around the conductor as it builds up, and so tend to cause the flash to take place elsewhere.

Mr. J. H. R. Nixon: Experience shows that the transmission lines which suffer most from flashover are 11-kV and 6·6-kV lines; the lower-voltage and higher-voltage lines seem less susceptible to damage. In the former case this is no doubt due to the position of the lines and to the margin of safety in the insulation, which is chiefly governed by mechanical factors, for the failure of the insulators depends on the follow-up of the power current subsequent to flashover and this does not appear to occur so readily on low-voltage lines as on those of 11 kV. The higher-voltage lines have insulators with high impulse breakdown voltage, and this is frequently high enough to withstand flashover during lightning surges.

I wish to enlarge a little on the author's remarks on the mechanism of the flashover on transmission lines, and to consider the effect on connected apparatus. When lightning strikes the line a surge is set up which travels in both directions. When this reaches an insulator with a lower impulse breakdown voltage than the amplitude of the surge, flashover results. Flashover may not in itself cause serious damage—it depends on the point of the wave of the power voltage at which flashover of the insulator occurs. For example, if a flashover occurs at a zero point of the voltage wave then the power current will not follow and no damage will result. If, however, the flashover occurs at the maximum point of the power voltage wave then the power current may travel along the path of the flashover, and damage to the insulator may result. The amount of damage will depend on the action of the protective gear.

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* See vol. 80, p. 21.

When the surge reaches connected apparatus such as a transformer or a machine, owing to the change of surge impedance, and to the relatively high frequency, the voltage momentarily piles up on the end turns of the transformer or machine. Has the author experimental or theoretical knowledge of the voltages that may so occur, and can he suggest a practical basis for calculating the magnitudes which are encountered?

If an overhead line is connected to an underground cable, protection is to some extent secured to apparatus at the end of the cable. Can the author give information as to the surge impedance of lead-covered cable and the extent of the protection thereby afforded against overvoltage on connected apparatus?

The paper contains various references to surge currents, and mentions magnitudes of 50 000 and 100 000 amperes. Some authorities have stated the current following from a lightning stroke to be of the order of half a million amperes. Will the author please indicate whether an upper limit of current has been established?

As stated in the paper, the deductions of surge impedance are little more than a guess. On that basis a structure of calculation is raised which gives voltages of the order of 12 million volts. I hope that the author will continue his researches to the point where results may be predicted with reasonable accuracy. The impression is that, as in the problem of the age of the earth, calculation gives figures of such high order of magnitude that errors of a few hundreds per cent are of no importance. It will, however, be a step forward if only the upper and lower limits can be established.

Mr. R. C. Woods: Some 20 years ago the company with which I was associated had a telegraph and telephone route over exposed moorland in the North of England which was continually subject to apparatus damage in thunderstorms. The difficulty of obtaining reliable pole earths was suggested as a contributory factor, and an experimental guard wire was run on the pole saddles and well earthed at both ends. No recognizable benefit was obtained over a period of 5 years, however, and the experiment was therefore abandoned.

Dealing with the question of proneness to lightning troubles, I would instance the case of a farmhouse lying in a small valley a few miles north of Derby, which has had one of its two stone chimney shafts struck seven times. This shaft has been rebuilt four times, yet the other shaft, of the same height, is immune. Local householders consider themselves safe, as only this one house has been struck within living memory. Can the author suggest an explanation?

In the open country of Norfolk—well-wooded farm land and forest—there is a saying that the only safe tree to shelter under in a thunderstorm is the hawthorn.

From the paper it would appear that a dangerous surge in a conductor, due to lightning, has a time factor of only a few microseconds. We are at times asked to provide, in addition to the telephone protective equipment, for special protection against lightning effects (arresters, chokes, and gas discharge tubes), a relay which will be operated by the functioning of these and will earth both lines. As a light relay will have a time-lag of, say, 5 milliseconds, and one capable of earthing large currents will have a much greater lag, the value of this device appears to be very doubtful. I should appreciate the author's opinion on this point. Unlike power lines, telephone lines employ no voltages much above 100 volts in service, so that the system itself will not contribute any power to follow up a flashover.

Mr. N. H. Bentley: In the photograph of a lightning flash taken by Dr. Allibone* the discharge appears to strike the steeple some distance from the point. Has the author any reason to put forward for this? Is it possible that the impedance of the air path was much less than that of the lightning conductor, owing to the inductance of the latter? On the right-hand side of the photograph there appears a rather curious looped flash, and I should like to have the author's explanation of this discharge.

The Boys camera is mentioned in the paper as having lenses revolving at a very high speed, whereas I gather from the author's description that a high-speed film was used in it; I should like to have a few more details of the camera.

Referring to the formation of leader strokes, what is the approximate duration of the ionized path, i.e. how long does it take for this path to decay if the charge has disappeared?

Mr. J. D. Pierce: There is one point of particular interest to me which the author passed over rather quickly—the question of the point where lightning is most likely to strike. He showed in one of his slides lightning striking a conductor halfway down. I have had a case to investigate quite recently which was somewhat similar to that of a tree struck at the base of the Liffel Tower, but on a smaller scale. My case occurred at one of the C.E.B. substations, where there is a great deal of steelwork, etc., all round, and there were three flashes in very quick succession: one of them hit the tower just outside the substation; the second went to earth on the structure; and the third hit an oak post about 40 ft. from the substation, 50–60 ft. from the base of the 60-ft. steel structure, splitting the post in two and throwing one part of it on to the access road. I should be very glad to have an explanation of this phenomenon.

Mr. J. F. Driver: Will the author give us his views as to the energy in a lightning stroke? Various investigators have given figures varying from 2·5 kWh to as much as 6 000 kWh. In view of recent experimental work it would be interesting to know what is now considered the probable figure.

The author has shown on the screen one oscillogram of a lightning stroke, the wave-shape of which appears to differ from that of the usual curves obtained in the laboratory from a normal impulse generator. A number of oscillograms have been published in America which are more like the "laboratory" shape. Is the photograph he has shown typical of lightning, or is it in the nature of a freak?

Transmission engineers would be glad to know what proportion of the surges on transmission lines arise from direct strokes and what from inductive disturbances due to lightning strokes at a distance.

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* Shown as a lantern slide at the meeting.

A theory has been advanced that animals, including man, to some extent ionize the air, rendering themselves more susceptible to lightning shock. It is certainly surprising how often people are killed in moderately open spaces, whilst in the surrounding district there are trees, buildings, and hills, which surely should offer a better target for the lightning stroke.

[The author's reply to this discussion will be published later.]

WESTERN CENTRE, AT BRISTOL, 8TH MARCH, 1937

Prof. David Robertson: Simpson's theory accounts for the generation of a difference of potential between parts of the atmosphere, whilst Wilson's only accounts for the amplification of one which already exists. It is probable that both actions play their part in the production of lightning. Wilson's picture, as given by the author, requires some modification. Attractions and repulsions by the induced charges on the falling drops can only have a small effect on the motion of the ions, for these drops can only increase the electric field in a ratio not greatly different from that of the spacing of the drops to the clear distance between them. But these attractions and repulsions are not necessary to the theory. Since the drops are falling, they will naturally collect more of the negative ions coming up to meet them than of the positive ions moving with them. Since the drops presumably start with a positive charge from the cloud, the Wilson action must first discharge them before they begin to acquire a negative charge. When a field exists below the cloud as shown in Fig. 1, the Wilson action will be reversed in those drops which fall out of the cloud, intensifying the p.d. between the cloud and the earth. These drops will be negative as they leave the cloud, will get discharged as they fall, and then become positive if the cloud is high enough. If the cloud be low, they will reach the ground before the negative charge has all been dissipated, but if it be high their charge will be positive. Hence, the sign of the charge brought down by the raindrops reaching the ground gives no sure indication of the charge on the cloud itself. If the field-intensity graph in Fig. 2 is intended to show that in the vertical line through the upright rod, it ought to show zero along the height of the rod. The author should explain what the arrows in Fig. 3 stand for. If it is the direction of the current, they should be reversed; or, if it is the direction of the propagation of the flash, should they not extend outwards in both directions from the centre where the flash started?

In connection with the bursting of timber and other moisture-containing materials when struck by lightning, the pressure of the steam generated by the heat of the current would appear to be sufficient to account for the observed facts.

The statement that all heavy metalwork in a building should be solidly bonded together must not be taken literally. In the case of a church the tower or spire to which the main lightning conductor is fixed is the part most likely to be struck, and each time it is struck very large voltages will exist momentarily between the rod and earth because of the impedance of the rod itself and the resistance of its earth connection. No other metalwork, other than external metal on the tower itself, should be connected to the main rod or to its earth plate, and, especially, the lightning-rod earth should on no account be used for earthing the electric conduits or other interior metal. If other external metal is also in danger of being struck, it should have a separate earth. Otherwise there is danger of flashing, with possible danger of fire, at unexpected places. I am acquainted with two instances in which alarming flashing was seen inside a building owing to a length of conduit having been earthed to the lightning rod. Fortunately there was no damage.

The theory of Section (1) (*a*) recalls a paper published in 1900 in which I showed that the adiabatic lapse rate was the limiting one for vertical equilibrium of the atmosphere.* As I was then interested not in thunderstorms but in the down-draught in a chimney, I did not get as far as the greater tendency to instability with moist air.

Mr. T. W. Mackay: I am satisfied to belong to the old school of thought, and regard lightning and electrically-charged cloud formation as being impressed from above, rather than accept that it is initially due to thermodynamic effects. I am inclined to favour the theory proposed by Dauzère, which was no doubt stimulated by the deductions and experimentations by Hallwachs, who found that ultra-violet rays had the power of causing the negative charge to leak away from the charged body. Ultra-violet light is an active agent in the upper reaches of space, and it is therefore rational to assume that it plays an important role in the formation of a thundercloud.

I agree with the author as to the existence of lightning trouble zones. I have in mind two quite separate areas of this sort, about 1 mile behind and approximately parallel to a river bank, on the side facing the prevailing wind. I am of the opinion that these trouble zones are not caused by the river, but are due to the particular formation of the river banks and the depth of subsoil over the rock formation. When intense evaporation takes place from the river, the banks add little to the total migration of aqueous vapour into the upper space, apart from assisting the upward movement by the heat radiation from the rockbound river banks. This upward movement, combined with the prevailing wind from a "cold front" against the cold dry air falling in at the bottom of the column, stretches diagonally upwards in the direction of the prevailing wind in a horizontally projected distance about 1 mile beyond the river. This effect is not so noticeable where the river is low-lying, where there is wet and moisty land, or under conditions where there is no abrupt change in the evaporation. It is reasonable to argue that the ionization will be greatest along the "cold front" plane, and that there is a great probability of a semi-conducting zone being the path of lightning discharges from the charged clouds.

With reference to the shattering effects of a lightning flash, I have had the opportunity of examining two 32-ft., 10-in. red fir, creosote-impregnated poles that had apparently taken a direct hit. The shattering was typical of an internal explosion, as it had burst the longitudinal fibres, leaving them looking very similar to a loosely hooped barrel. The maximum radial displacement had taken place about half the height of the pole above ground. The disruption was no doubt due to a combination of thermodynamic and electrical events, the first caused by vapour pressure in the core of the pole and the latter by the mutual reaction of the current from the core of the outer boundaries of the pole.

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* "On the Equilibrium of a Column of Air and the Atmospheric Temperature Gradient," *Proceedings of the (Royal) Philosophical Society of Glasgow*, 1900, vol. 31, p. 145.

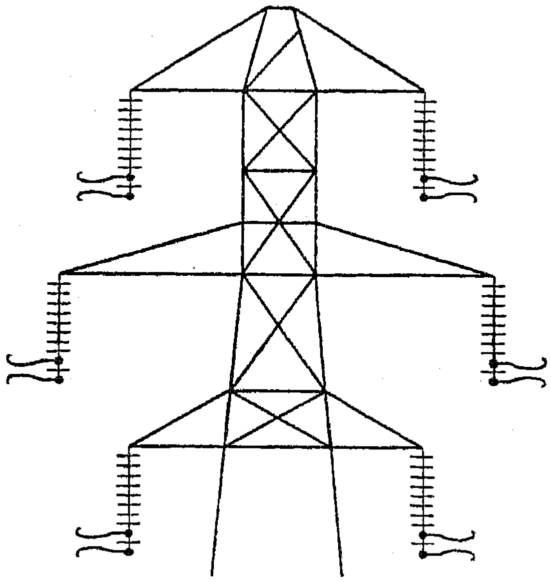

Fig. J

In dealing with the theoretical side of his subject the author assumes fixed values for the line constants, despite the facts that the resistance of any conducting medium increases very rapidly with frequency and that corona effects enter into the problem at voltages depending on the physical dimensions of the lines, between 200 and 300 kV. In each of the two cases the energy loss is non-reversible and thus influences the velocity of propagation of waves in wires to a marked degree. I am particularly anxious that full recognition be given to these facts, since my own method of surge-arresting, which I am about to describe, depends entirely upon the resultant effect of displacement and attenuation of waves in parallel lines having different prescribed constants.

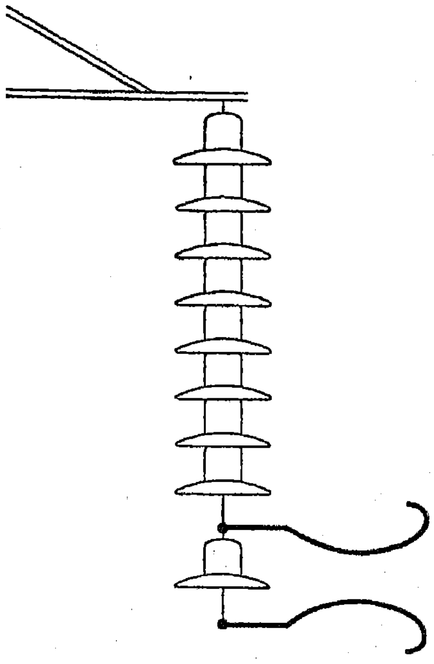

Fig. K

Heaviside's analysis of the propagation of waves in wires and cables gives an equation of the form

$$E_x = E_1\epsilon^{-\alpha x}(\cos \omega t - \beta x)$$

where α is known as the attenuation factor and β as the wave factor, and each is a function of L, C, G, and K. This equation is based upon the assumption that the wave obeys the harmonic cycle, which is, of course, open to grave doubt; nevertheless, it serves to illustrate clearly my theory. The method of surge-arresting which I would put forward consists in introducing a second line, insulated from, and below, the original line, on the same string of suspension insulators, as shown in Fig. J. Fig. K illustrates in more detail the arrangement; it is anticipated that the elementary gap will be effective in dissipating the extraneous surge energy. Fig. L represents propagation curves for a surge voltage applied to such an arrangement. Initially the voltage amplitude is the same for the two conductors at any point, but owing to the parallel line having widely different constants the amplitude of the decrement and also the phase relationship becomes different for each conductor. Thus a voltage-difference, as illustrated in Fig. M, results, and this is, in effect, a measure of the surge energy.

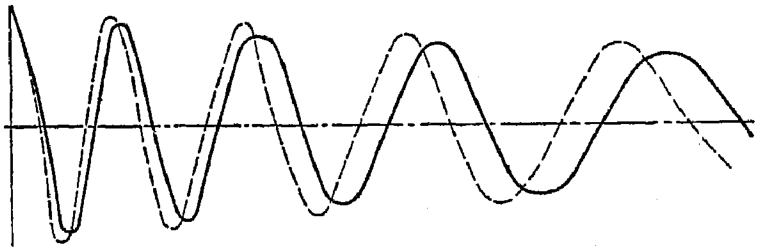

Fig. L

The above general treatment may be criticized since a doubtful harmonic function for the propagation has been assumed; but even if the wave voltage is derived by equating the power dissipated in an elemental length of line, namely

$$ri^2\,dx + e^2g\,dx \text{ watts}$$

the value e_x is given by

$$e_1\epsilon^{-\frac{1}{2}}\left(\frac{r}{z} + gz\right)^x$$

and it will be noted that the rate of change of the above is

$$\frac{de}{dx} = -\frac{z}{2}\left(\frac{r}{x^2} + g\right)e$$

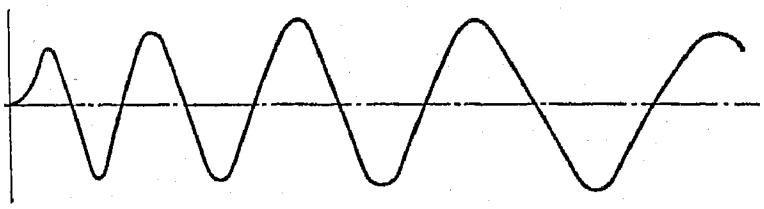

Fig. M

Thus, if the line constants are different, the rate of change of the voltage-rise is again different in the respective lines, and a difference of potential is obtained across the horn gaps.

It should be noted that under normal load conditions each parallel phase line will accept load current in inverse proportion to its impedance. A further interesting point is that the extraneous surge energy is radiated from the lines across the gaps. It is not radiated to an earth circuit, and therefore cannot draw a power arc. I am not in a position to carry out extensive tests on the foregoing idea, but I recommend it to anyone sufficiently interested to find out whether such an arrangement is effective.

I would ask the author whether he thinks the Petersen coil is effective in suppressing flashes to earth in an interval of 5 to 15 microseconds, since all inductive apparatus behaves, over the first interval of a surge, like a condenser. In the case of the Petersen coil I submit that the reactive voltage will not have had time to build up to oppose the capacitance effect of the two sound lines in vector combination. I have studied oscillograph records of the functioning of a similar equipment, the compensated reactor, and although both equipments are effective in dealing with any power arc that tends to follow the initial " spit-over," I hesitate to believe that either can function in sufficient time to deal with transient surge absorption.

Mr. R. W. Biles: The engineer is concerned with the means for diminishing the effects of lightning discharges upon power supply systems, and in this connection the author's comments upon tower footing resistances are helpful. Recent experiences, however, tend to show that towers having resistances lower than 10 ohms, with 9 cap-and-pin insulator units, are vulnerable to flash-over, there being little or no distinction between them and towers with higher footing resistances. I believe the E.R.A. are concentrating their attention upon determining the surge impedance of the tower itself.

The author refers to the existence of danger spots, and I think it can be agreed from observations which have been made that certain localities are more prone to lightning discharge than others; in particular, in the S.W. England and South Wales area, judging from the records of flashover on 132-kV and 33-kV lines, the land in the vicinity of the north and south coasts of the Bristol Channel appears to have been affected more than other parts. On the other hand, the effects of lightning appear to be much less in Cornwall, where experience over the past 3 years shows the grid system to be practically immune from such troubles.

In view of the obvious damage which might occur to expensive terminal apparatus, certain precautions have been tried with a view to improving security of supply, namely the fitting of co-ordinating gaps, to allow the sparkover to occur outside a transformer rather than inside. These have proved of value in many instances. The additional precaution is sometimes taken of switching out one transformer of a pair on the approach of a thunderstorm. Expulsion gaps have been applied at a number of points, but the results have been of a mixed nature.

The author does not refer to the effect of indirect strokes, and I think it is obvious that if devices can be applied which will deal with the discharges occurring from strokes of this type, which in almost every case cause no damage whatever to a transmission system, we shall go a long way to removing most of our difficulties.

Mr. W. C. Bowler: The subject dealt with in the paper is being tackled on the Continent to a very great extent, as we found when the Transmission Section visited France last year. We were informed by the French engineers that they had done and were doing a tremendous amount of experimental work in connection with this subject. M. Ailleret informed us that three of the bulk-supply concerns had co-operated in this matter and had spent £5 000 in a mobile outfit for making tests throughout the country. Their experience was that certain types of ground, owing to emanations, were more liable to lightning than others. They found that a transformer substation which had constantly been struck by lightning, was freed from further lightning trouble by being moved some 700 yards to another position. It is now their practice, in planning grid lines, etc., to make a survey with the travelling laboratory and build the line in accordance with the results of the experiments carried out. I should like to ask whether any similar tests have been carried out in this country.

Mr. R. C. Golding: I should like to ask whether the author has noted any phenomena which would tend to suggest that a live line possesses any particular property of attracting or, in the opposite direction, initiating a lightning discharge. There is no doubt that the incidence of lightning strokes in a given district hitting or affecting lines is higher than the proportion of the total storm area covered by that line, and it would seem to be possible that, owing to some static effect of the line, this might at least tend to offer some attraction for the stroke.

From the author's photographs of impulse discharges it appears that it is not difficult to arrange for these to flash-over continually to a spot located within very narrow bounds, and some interesting results should be obtained by bringing a highly insulated wire close to the path of the discharge and measuring the surges set up.

With regard to the possibility of geological structure affecting the path of a direct stroke, evidence supporting this was given in the case of an 11 000-volt line passing over the crest of a hill down into a valley. When the line was struck, the flash-over regularly took place about a quarter of the way down the slope—missing the line over the crest—even after the affected section had been fitted with 20 000-volt insulators.

I doubt whether tower footing resistance, within the limits found in practice to-day, has much effect on lightning incidence. I instance the occasion when I viewed the remains of an "A" pole which was fitted with a lightning conductor, where the resistance down one leg to earth after being struck was 6 ohms. The evidence pointed to the lightning stroke having followed the following path: hitting the point of the lightning conductor, it followed this down for about 18 in., then passed along the securing bolt at the apex of the pole—to the leg remote from the conductor. This it split downwards, till it reached the anti-climbing guard of barbed wire. Following this in a spiral round the pole, leaving burns, it continued down the pole to within 2 ft. from the ground, where it apparently jumped 3 ft. to a wire fence on reinforced-concrete poles—finally destroying a pole some 20 yards away.

[The author's reply to this discussion will be published later.]

THE AUTHOR'S REPLY TO THE DISCUSSION BEFORE THE INSTITUTION

Prof. B. L. Goodlet (*in reply*): My aim in writing this paper was to cover all aspects of the subject in a provocative manner, so as to draw expressions of opinion both from practical engineers and from physicists and others interested in lightning, mainly from the scientific standpoint. I consider that this object has been attained; I thank all contributors to the discussion and I apologize for my inability to reply adequately to many of the questions raised.

The remarks of Sir George Simpson, Dr. Whipple, Mr. Watson Watt, Mr. Scrase, Mr. Lutkin, and Sir Charles Boys, are valuable contributions to knowledge. As Sir George Simpson says, the results of his observations set at rest several of the problems discussed in the paper, and I hope I may be permitted to express my admiration for the direct and masterly way in which he and his assistants have attacked the difficult problem of the electrification of thunderclouds. If, however, it is correct that these observations have failed to detect any strong electric fields, I hope that they will be continued, because it appears to me incredible that a lightning flash can be initiated otherwise than by a strong electric field.

Sir Charles Boys's work on lightning is well known; I am particularly interested in the observation on ball lightning quoted. Water "too hot to put one's hands in" is probably over 60° C.; if the initial temperature was 10° C. we get for the minimum energy 2 000 lb.-deg. C. or 3 800 kW-sec. If we assume that all the water in the butt was raised to 100° C. and that 4 lb. of water was actually evaporated, the energy becomes 10 940 kW-sec., which is identical with Prof. Thornton's estimate of the energy of a fireball—viz. 8 million ft.-lb. I find it hard to account for such a large amount of energy on Mr. Kilburn Scott's nitrogen-dioxide hypothesis, but I am a bad chemist and it may be possible to do so. I leave the problem to persons better qualified than I am to decide.

Dr. Miller suggests that the step development of the leader stroke may be due to cascading in the cloud. The possibility occurred to me when writing the paper, but I consider the explanation I gave verbally to be more satisfactory. In reply to Dr. Miller's remarks on the influence of tower surge-impedance and rate of rise of current on the tower-top potential, I would say that when footing resistances are very low I agree that the tower surge-impedance determines the tower-top potential. I would, however, remark that if tower surge-impedance was *always* the controlling factor the number of flashovers would not be decreased by reducing footing resistances, whereas practical experience shows that reduction of footing resistances is usually beneficial. In estimating the probability of flashover, the duration of a voltage peak as well as its magnitude must be considered, and the high potentials produced in this way last for only small fractions of a microsecond.

Dr. Miller's suggestion regarding the production of steep-fronted waves by slow-fronted lightning strokes is most interesting. The surprising thing to me, however, is not that there should be discrepancies such as he mentions, but that anyone should imagine that cathode-ray oscillograms of transmission-line surges are in any way an image of the wave-form of the stroke itself. Suppose, for example, that the current in a stroke rises to a maximum of 50 000 amperes in 5 microseconds; if this stroke falls on a line conductor of 500 ohms surge impedance a wave of front slope 2 500 kV per microsecond will be propagated down the line in both directions. If flashover occurs when the voltage reaches 2 500 kV, a cathode-ray oscillograph connected to the line will record a wave of only 1 microsecond front; only in the rare case of a very weak stroke which does not cause flashover will the cathode-ray oscillogram be a true record of the stroke wave-form. If the stroke falls on a tower or earth wire, an oscillograph connected to a line wire can record only induced voltages, or voltages resulting from flashover. In the latter case the "wave front of the lightning surge" will be determined by the time of breakdown of the flashover path concerned! This is, of course, the case to which Dr. Miller's three oscillograms refer. Considerations of this kind have led me to a conclusion rather at variance with many peoples' ideas—that if one wishes to obtain reliable information regarding the magnitude and time variation of current in lightning strokes the last place to look for it is on a transmission line!

In reply to Mr. Mortlock, I would say that I regard his suggestion as quite reasonable, and I have made use of this idea both in my verbal exposition of the paper and in explaining shocks received from metal objects when a flash strikes close by (see page 10, last sentence in first column).

The theory in connection with 6(*b*) is given for a unit function wave, and the modifications needed to take into account a sloping wave-front are hardly worth mentioning. Most of the diagrams in the paper have, indeed, been drawn for both cases.

It remains for me to reply to my critics. Mr. Wilkinson takes me to task about the surge impedance of the lightning channel. If he will refer to pages 12 and 18 of the paper he will find that I have at least not wandered blindly into error—indeed his remarks read almost like a paraphrase of my own. I would point out that the surge impedance of the lightning channel does not play a very important part in the analysis, because I have not discussed the propagation of waves along the channel itself. All I have assumed is that the channel feeds current into the point struck and that waves returning to this point experience reflection; the surge impedance of the channel certainly affects the magnitude of the reflection coefficients, but not very much unless the channel impedance is much smaller than the other impedances concerned. I would also point out that the analysis is equally valid if the channel reacts as a pure resistance. Finally I would ask Mr. Wilkinson whether he can suggest any other way of attacking the problem and whether my way is at least not better than that adopted by many previous writers, which is to discuss the travelling waves produced by lightning and ignore completely the manner of their formation.

Mr. Norris's remarks read rather like an attack on the use of mathematics in the solution of physical problems. If this interpretation is correct, I will leave the reply to an abler pen and ask him to read paragraphs 8, 9, and 10, of Oliver Heaviside's "Electromagnetic Theory" (vol. 1, pp. 7 to 9).

In regard to the use of the most frequent value of a quantity (which is *not* usually the average value) as a basis for design it seems to me that this practice has much to recommend it. Use of the maximum value has to be justified on the economic basis of the relative cost of an occasional failure compared with the capital investment necessary to prevent it.

Mr. Taylor does not believe that ionization near ground-level can be produced by point discharges from earthed objects in the field of a thundercloud. He has evidently not read reference (6) of the paper. His next remark suggests that he has never heard of the dark discharge (see, for example, Whitehead's "Dielectric Phenomenon," vol. 1, p. 155). His suggestion that ionization under a thundercloud is due to the descent of electrified air or heavy ions is surely not based on the idea that heavy ions settle under gravity? His ideas in this matter are not expressed sufficiently clearly for me to be able to reply.

THE PERFORMANCE OF NOISE METERS IN TERMS OF THE PRIMARY STANDARD

By B. G. CHURCHER, Member, and A. J. KING, B.Sc.Tech., Associate Member.

(*Paper first received* 19*th August, in revised form* 18*th December*, 1936, *and in final form* 21*st April;* 1937; *read before the* METER AND INSTRUMENT SECTION 12*th February*, 1937.)

SUMMARY

The paper discusses the performance of noise meters, such as are used for engineering measurements, in terms of the primary standard of "Equivalent Loudness" which has recently been formulated. The unit of equivalent loudness is the British Standard Phon. The technique for the basic determination of the equivalent loudness of a noise is discussed, and the Phon values for a number of specific noises of importance in engineering are given. The fundamentally important equal-loudness relations for pure tones of differing frequency are considered, and the results of some direct determinations in the free field are given. The performance of subjective noise meters is then examined for noises of various types. A meter using two telephone receivers, which offers some advantage over the more usual types, is described. The performance of the objective type of meter is also examined. The measurement of noise associated with irregular sound fields is briefly discussed.

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  (*b*) Physical conditions.
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  (*d*) Specimen equivalent-loudness measurement.
(4) The Equal-Loudness Relations.
(5) The Equivalent Loudness of Certain Physically Specified Noises.
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  (*b*) Two-telephone subjective meter.
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  Appendix III. Extended Free-Field Equal-Loudness Relations.

(1) INTRODUCTION

During the past few years much data and experience in the intricate subject of noise measurement have accumulated and a clearer grasp of essentials has been obtained. However, the interchange of ideas and data, and hence progress in the art, has been hampered by a lack of authoritative definition of the various terms and units employed, particularly with respect to loudness. To electrical engineers this is an important matter, since the measurement of noise arising in engineering is essentially the measurement of the loudness of a complex sound. Since loudness is a sensation and not a physical magnitude, its definition and measurement involve subjective considerations, such as the manner in which the sound is heard. Until such factors were specified, a foundation could not be laid upon which a consistent and dependable system of noise measurement adequate for engineering purposes could be built. Hitherto, apart from terminological confusion, each type of portable meter which may be used for engineering measurements has had its own peculiar and arbitrary basis of calibration. Hence, though the term "decibel" may be used for each, the indications of different types of meter may differ considerably for a given noise.

An important step towards removing these difficulties has been achieved by the work of the British Standards Institution. An authoritative glossary[*] of acoustical terms and definitions is now available, to which it is hoped all workers in the field of acoustics in this country will conform. During the formulation of the definitions, consultation took place with other national bodies with a view to eventual international agreement. The definitions provide, among other things, the basis of a primary standard for noise measurement, in terms of which noise meters of any type may be calibrated. A new noise unit, the British Standard Phon, has been adopted, the term denoting that a noise value so designated has been derived, directly or indirectly, in accordance with the specified conditions.

(2) THE BRITISH STANDARD PHON

Loudness is defined as: "That subjective quality of a sound which, in general, increases regularly with the intensity within the limits of audibility." A true or natural loudness scale is such that when the number of units on the scale is doubled (or trebled), the magnitude of the sensation experienced by normal listeners is also doubled or (trebled). The existence of such a scale has been established by the correlation[†] of experimental data published by independent investigators who have sought to discover the scale.

Fundamentally, the magnitude of a noise is expressed by the magnitude of the standard reference tone which to a normal listener sounds equally loud. It would therefore seem natural to quote the loudness of the reference tone as a measure of the noise magnitude. The numerical values assigned to noises of different magnitudes are then in accord with the magnitudes of the loudness sensations evoked in normal listeners. In many practical engineering problems it is advantageous to express noise magnitudes in this form.

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[*] Glossary of Acoustical Terms and Definitions, B.S.S. 661—1936.

[†] B. G. CHURCHER: *Journal of the Acoustical Society of America*, 1935, vol. 6, p. 216; also H. FLETCHER: *Journal of the Franklin Institute*, 1935, vol. 220, p. 405.

However, the magnitude of the reference tone is ascertained initially from its observed physical intensity, which is conveniently expressed on a logarithmic scale. Thus the intensity level is expressed as the number of decibels above a stated reference intensity. In technical work it is often sufficient to express noise magnitudes in terms of reference-tone intensity-levels without converting them to loudness values. The numerical values thus obtained do not correspond even roughly to the sensation magnitudes experienced, as the value of the logarithmic scale lies in its mathematical and instrumental convenience rather than in its relevance to the laws of audition.

The adoption of the term " Equivalent Loudness " for the physical intensity level of the reference tone enables it to be distinguished from loudness. It is defined as follows:—" The equivalent loudness of a sound is measured by the intensity level relative to some accepted reference intensity of a standard pure tone of specified frequency, which is judged by a normal observer to be as loud as the sound."

The unit of equivalent loudness, the phon,[*] is defined as follows:—

" The standard tone shall be a plane sinusoidal sound wave train coming from a position directly in front of the observer and having a frequency of 1 000 cycles per sec.

" The listening shall be done with both ears, the standard tone and sound under measurement being heard alternately. . . . The intensity level of the standard tone shall be measured in the free progressive wave. The reference level shall be taken to be that corresponding to an r.m.s. sound pressure of 0·0002[†] dyne per cm^2.

" When, under the above conditions, the intensity level of the standard tone is n decibels above the stated reference intensity, the sound under measurement is said to have an equivalent loudness of n phons."

It is important to note that the definition envisages a normal mode of listening, i.e. with both ears while facing the source, and avoids abnormal conditions, such as that of listening to two sounds simultaneously. Hence the conditions chosen may be expected to lead to results which are valid for practical purposes. Further, the conditions, while not corresponding to those adopted by the German Committee, correspond closely to those adopted by the U.S.A. Committee,[‡] thus making British and U.S.A. equivalent-loudness (E.L.)[§] data directly comparable. So far as the authors are aware, the precise relation between German and British bases has not yet been ascertained. The alternate listening specified in the definition of the phon requires means for quickly starting and stopping both reference tone and noise. The production of a plane wave proceeding in a definite direction implies the absence of material reflection, which calls for the use of a heavily absorbent test room such as is seldom available outside a laboratory. Hence, with the exception of small purely electrical apparatus which can be switched on and off, the direct measurement of the E.L. of engineering apparatus is, in general, impracticable.

However, noise meters suitable for engineering measurements may be calibrated in phons in the laboratory as follows: Reproducible noises of physically specified composition are set up by means of a loud-speaker and their phon values determined in accordance with the definition. These same noises may then be applied to noise meters of any type, the indications of which are noted. The discrepancy between an instrument indication and the phon value is the meter error for the type and level of noise applied. The experimental noise may be that of an actual engineering apparatus, e.g. motor or transformer, reproduced on the loud-speaker by means of a microphone and amplifier, the engineering apparatus being, of course, located in another room. But from the point of view of the calibration of portable meters such a course is not, in general, necessary. The performance of the ideal noise meter would be independent of the type and level of noise over a wide range, in the same way that a satisfactory a.c. voltmeter indicates correctly over a wide range of frequency and wave-form. In the calibration of subjective meters there is no need to maintain fine distinctions between the tone structures of the various noises met with in engineering practice, and a few well-contrasted noises specified in physical terms is sufficient. This is less true for objective meters, as will be seen later. Data accumulated from sound analyses on engineering apparatus enable noises to be selected for calibrating purposes.

This plan has several advantages. The physically specified standard noises can be set up synthetically (see Appendix II) by means of convenient laboratory devices rather than by using engineering apparatus. The degree of constancy, reproducibility, and flexibility of control, of synthetic noises set up in a non-reflecting test room can far exceed what is possible with engineering apparatus operating in normal circumstances. For basic measurements these are important aids to precision. In the routine calibration of noise meters the E.L. of the physically specified noises would have to be determined once only, after which the basic technique would not be required. Incidentally, the plan enables not only the calibration of noise meters on an agreed basis, but also the intercomparison of the phon values of the several selected noises obtained by different laboratories to be carried out, thus strengthening the foundations of the system of measurement.

(3) THE TECHNIQUE OF THE BRITISH STANDARD PHON

(a) Subjective conditions.

Equivalent loudness is defined in terms of the loudness-equality judgment of a hypothetical " normal observer," whose loudness judgment is representative of that of people in general. In practice this judgment can only be inferred from the judgment of a group of persons. The larger and more comprehensive the group the more closely will the group judgment approximate to that of the "normal observer." Thus a group of 100 persons covering a wide range of ages and occupations and of both sexes would enable ideal conditions to be approached. But, as will be shown, conditions sufficiently representative for most purposes may be obtained with a group of 10 persons.

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[*] The phon here defined differs slightly from that originated in Germany, the latter embodying listening with one ear and a reference pressure of 0·000316 dyne per cm^2.

[†] More accurately an intensity level of 10^{-16} watt per cm^2, corresponding to a sound pressure of 0·000204 dyne per cm^2 at 20 deg. C. and 76 cm. of mercury. This value also approximates to the 1 000-cycle threshold pressure for a normal observer.

[‡] " Proposed Standards for Noise Measurements," *Journal of the Acoustical Society of America*, 1933, vol. 5, p. 109.

[§] This abbreviation is employed throughout the paper.

The best means of obtaining the group judgment within the terms of the definition has now to be considered. The procedure used should be applicable to any normal person in order that fully representative data may be obtainable. Hence no technical knowledge or skill should be presupposed. The procedure should permit the observers to perform their essential task of judging relative loudness in a simple and natural manner, without distraction, constraint, or biasing influences. Two general procedures are used in psychophysical measurements. One is known as the method of Constant Stimuli (C.S.),* and the other as the method of Average Error (A.E.).* A comparative study of these methods has been carried out by W. N. Kellog,† both for sound and light measurements. In A.E. applied to equivalent loudness measurement, the observer himself adjusts the intensity of the reference tone (R) to what he judges to be loudness equality with the noise (N). In C.S. he is only required to judge which of the two successively presented sounds predominates in loudness, the reference-tone adjustments being performed by an operator outside the test room. This observation is repeated for a number of reference-tone intensities until the region in which the transition from predominance of N over R to predominance of R over N takes place is defined. It would appear that the directness and simplicity of A.E. afford considerable advantage over C.S. But, as Kellog points out, the observer's task in A.E. is to find equalities, whereas in C.S. it is to find differences. The less painstaking observer will easily satisfy himself that he has found equality, whereas the procedure of C.S. stimulates him to persevere in his search for differences and so to give more discriminating judgments. The observer's manipulations of R in A.E. have been found to disturb his judgment. Also the sustained listening to the alternating sounds necessary to arrive at a final setting in A.E. may induce fatigue, which further disturbs the judgment, whereas in C.S. the presentation of the two sounds for one or two seconds each, followed by a rest of, say, 10 seconds before the presentation of the next pair, minimizes fatigue. As a result of his investigations Kellog concludes that, for such a purpose as that under discussion, C.S. is capable of more reliable results than A.E.

Fletcher and Munson‡ in their work which has been used as a basis for the recommendations of the U.S.A. Committee, have, following Kellog, developed C.S. in preference to A.E. The present authors have carried out a comparison of C.S. and A.E. with 6 different noises, some sustained and some intermittent, using the same group of 10 observers. The agreement between the group judgments is very close, the differences between C.S. and A.E. varying from 0·5 to 2 phons. Taking account of the sign of the difference, the mean discrepancy for the 6 comparisons is 0·4 phon. Hence it may reasonably be concluded that C.S. and A.E. lead to the same phon values. The main difference between C.S. and A.E. was found to lie in the dispersions obtained, as expressed by the total spread of the judgments and standard deviations. While the value obtained for standard deviation depends to some degree on how it is defined, the values obtained for A.E. were always higher than those for C.S., in one case more than twice. Taking the 6 tests, the standard deviation for A.E. was, on an average, at least 60 per cent greater than for C.S. On a basis of the total spread of the observations, the dispersion with A.E. was also on an average 60 per cent greater than for C.S. In these comparisons the same observers were employed and any variations in the physical conditions were quite negligible. The only change made was the substitution of C.S. for A.E. Hence the consistent reduction in dispersion is regarded as confirming that the observers are better able to perform their task under C.S. than under A.E., and that A.E. contains undesirable elements not present in C.S. It was found that the average time required per observer was 5 minutes with A.E., compared with 6 for C.S. This was with 20-sec. rest periods for the latter, but 10-sec. periods have since been found sufficient. This reduces the time to 4·5 minutes for C.S. The time required for a complete test also depends on other factors, such as the way the work is organized. In any case, speed of working is secondary to avoidance of uncertainty. The smaller dispersion is of practical importance in basic measurements, because the probable error is directly proportional to the standard deviation and inversely proportional to $\sqrt{(n-1)}$, where n is the number of observers. Hence, in order to keep within a given probable error, a substantially greater number of observers is needed with A.E. than with C.S.

In view of the importance of eliminating psychological factors of uncertain effect, and the strong and independent evidence in favour of C.S., the authors have adopted it for their basic E.L. measurements. Thus, although it is a group loudness-equality value that is sought, it is considered to be obtainable more surely through judgments of loudness predominance performed by the individual observers than by direct equality judgments.

In C.S. the important principle of placing the control and recording of the noise and reference tone values in the hands of an operator located outside the test room obviates bias on the observer's part, since he has no control over or knowledge of the physical values of the sounds presented to him. N and R are presented by means of a loud-speaker, in front of which the observer is seated. He signifies his judgment of which of the two sounds heard in quick succession is the louder by pressing a switch operating a light signal visible to the operator. He is not permitted to give a decision that the sounds are equally loud. The process is repeated, with 10-sec. rest periods, several times, N being held constant and R varied over a suitable range in the vicinity of the observers' transition levels. In order to break up any speculation on the part of the more thoughtful type of observer as to what might be the " correct result," the intensities of the R presentations are varied in an irregular manner. Presentations at a given R level are made in the order N, R and R, N so that any psychological effect of the order of presentation may be eliminated. From the observations, a table is drawn up giving the judgment of each observer at each R level. Predominance of N is counted as 100 votes and of R as 0. Confirmed judgments, as obtained by check presentations, are awarded 100 or 0 as the case may be. Contradictory judgments at the same level are counted as 50 votes. The mean or group judgment at each R level is then computed. This ranges from 100 to 0 in favour of N being the louder. The corresponding distribution curve of group loudness-equality judgments may then be deduced. For this purpose loudness equality is interpreted as maximum uncertainty as to which is the louder. This uncertainty is clearly when the voting is 50 per cent in favour of each being the louder. The votes for equality at each R level are therefore obtained by noting the amount by which the mean exceeds 0 or falls short of 100 at these levels. If plotted, these values give a distribution curve of votes for equality with values of R, rising from 0 to a maximum of 50 and back to zero. The mean value of this curve gives the level of R corresponding to the mean group judgment of equality with N, and the peak of the curve defines the modal value. Mean and modal values coincide with a symmetrical curve. If j_1 is the number of votes for equality at level R_1, and J the total number of votes for equality, the mean E.L. value in phons is

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* These abbreviations are employed throughout the paper.

† W. N. Kellog: *Archives of Psychology*, 1929, vol. 17, no. 106.

‡ H. Fletcher and W. A. Munson: "Loudness, its definition, measurement and calculation," *Journal of the Acoustical Society of America*, 1933, vol. 5, p. 82.

$$R_e = \frac{(R_1 j_1) + (R_2 j_2) + (R_3 j_3) + \ldots (R_x j_x)}{J}$$

The calculation of the standard deviation is discussed in Section 3(*d*), where an example of an equivalent-loudness measurement is given.

It is important to note that C.S. makes a minimum demand on the observer; no technical knowledge or skill is required. Hence fully representative E.L. values are obtainable, the technique being applicable to any normal individual.

A matter requiring consideration is the magnitude of the steps in which R is varied. These should be small enough to enable the observer to exercise such discrimination as he can, but not so small and numerous as unduly to prolong the observations, thereby inducing fatigue. It is found that observers can frequently discriminate to 1 phon, so 1-phon steps have been adopted. If the size of step is not well below the observed standard deviation, the latter will differ from the true deviation. Sheppard's correction was evolved to allow for this point.* If S is the true standard deviation, S_1 the observed standard deviation, and W the size of step, then

$$S^2 = S_1^2 - \frac{W^2}{12}$$

Thus with 1-phon steps S does not differ materially from S_1 for values of S of 2 or more, such as are obtained with C.S. When the steps are large compared with S_1 the expression ceases to hold. The observers' judgments are no longer unconstrained. The observed standard deviations become strongly influenced by fortuitous circumstances unconnected with the observers' judgments.

The setting-up of momentary noises when substituting N for R, or vice versa, due to the transient characteristics of the sound source, is to be avoided. For this purpose the authors have devised a screened double potentiometer which enables the operator to make a smooth transition. The operation of the switch is timed by an electric clock with a large hand making one revolution in 2 seconds.

In determining an observer's judgments the operator first signals to the observer by means of a light signal that the presentations are about to begin and switches on one sound, which is maintained at the full intensity for the stipulated period. He then switches over quickly to the other sound and, after the same period at full intensity, switches off. Usually the observer has signified his decision before the expiration of the second tone presentation. The experience of the authors and others shows that the duration of the full intensity period is not at all critical. Some experimental results on this point appear in Table 4. Periods from 1 to 2 seconds are suitable, the longer period being advisable for very intermittent sounds. The authors have adopted approximately 2 seconds for general purposes.

The experimental procedure adopted minimizes fatigue, and the method of plotting the judgments enables appreciable fatigue to be detected. No observer is called upon to give more than two sets of judgments on the same day, and is given a rest period of at least 5 minutes outside the test room between two sets.

(b) Physical Conditions.

The definition specifies for the reference tone a plane wave coming from a position in front of the observer. In the present E.L. measurements, a highly-absorbent test room† 24 ft. × 24 ft. × 13 ft. high has been used, which enables reflected waves, i.e. waves proceeding in undesired directions, to be kept small in amplitude. One way of obtaining the plane wave is to use a substantially point source at a distance from the observer sufficiently great for the angle subtended at the source by the width of the head to be negligibly small. This may be advisable for high frequencies. For moderate and low frequencies a folded horn type of source has two advantages, viz. that a substantially plane wave may be secured without an unduly great linear distance (and hence without an unduly large test room), and tones of adequate intensity and purity are obtainable. It is advantageous to place the source at a level corresponding to half the height of the room, so that approximate symmetry is secured. The set-up is shown in plan in Fig. 1A. An observer's chair of suitable height and constructed so as to cause a minimum disturbance

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* YULE: " Theory of Statistics."

* B. G. CHURCHER: " The Acoustics Laboratory of the Metropolitan-Vickers Co.'s Research Department," *Engineering*, 1933, vol. 135, p. 563.

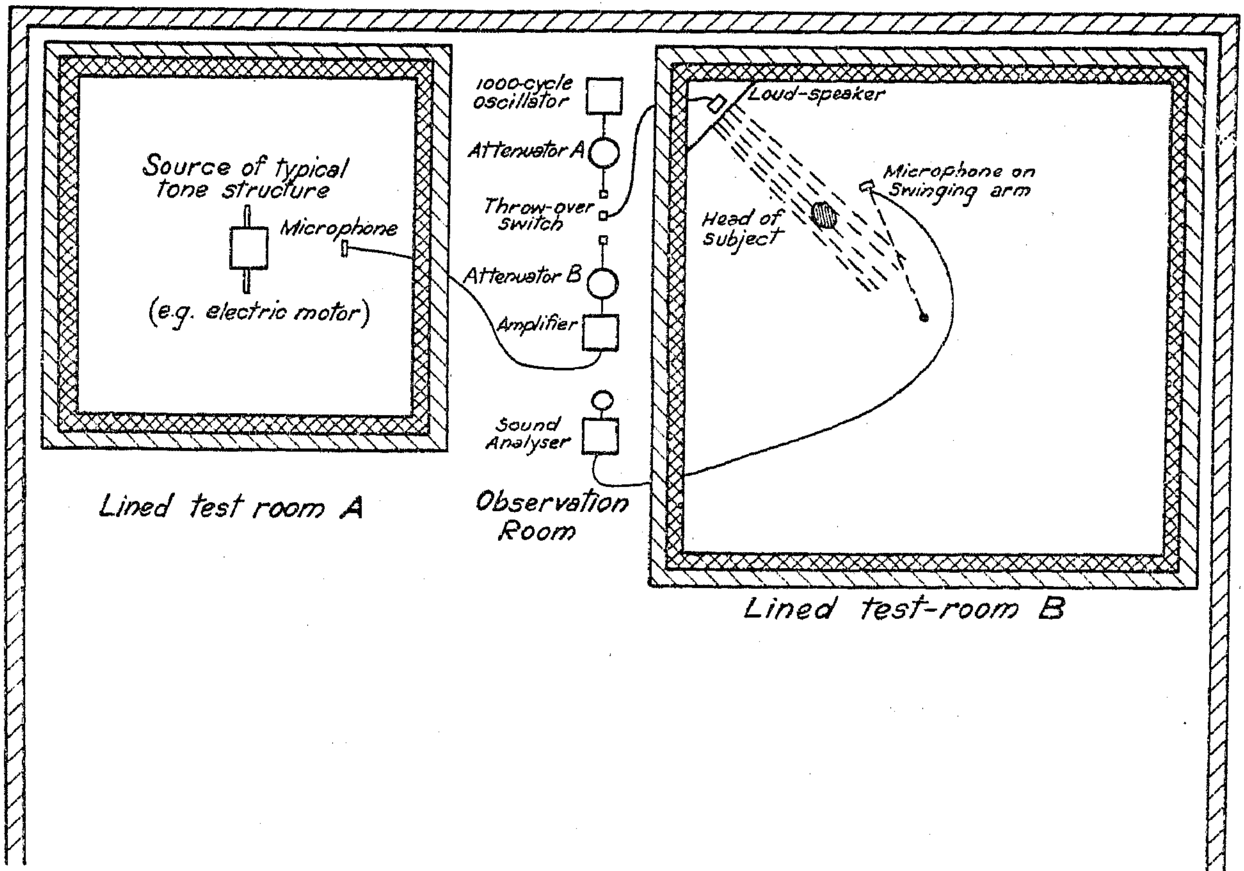

Fig. 1A.—Set-up for the calibration of noise-measuring instruments in terms of primary standard.

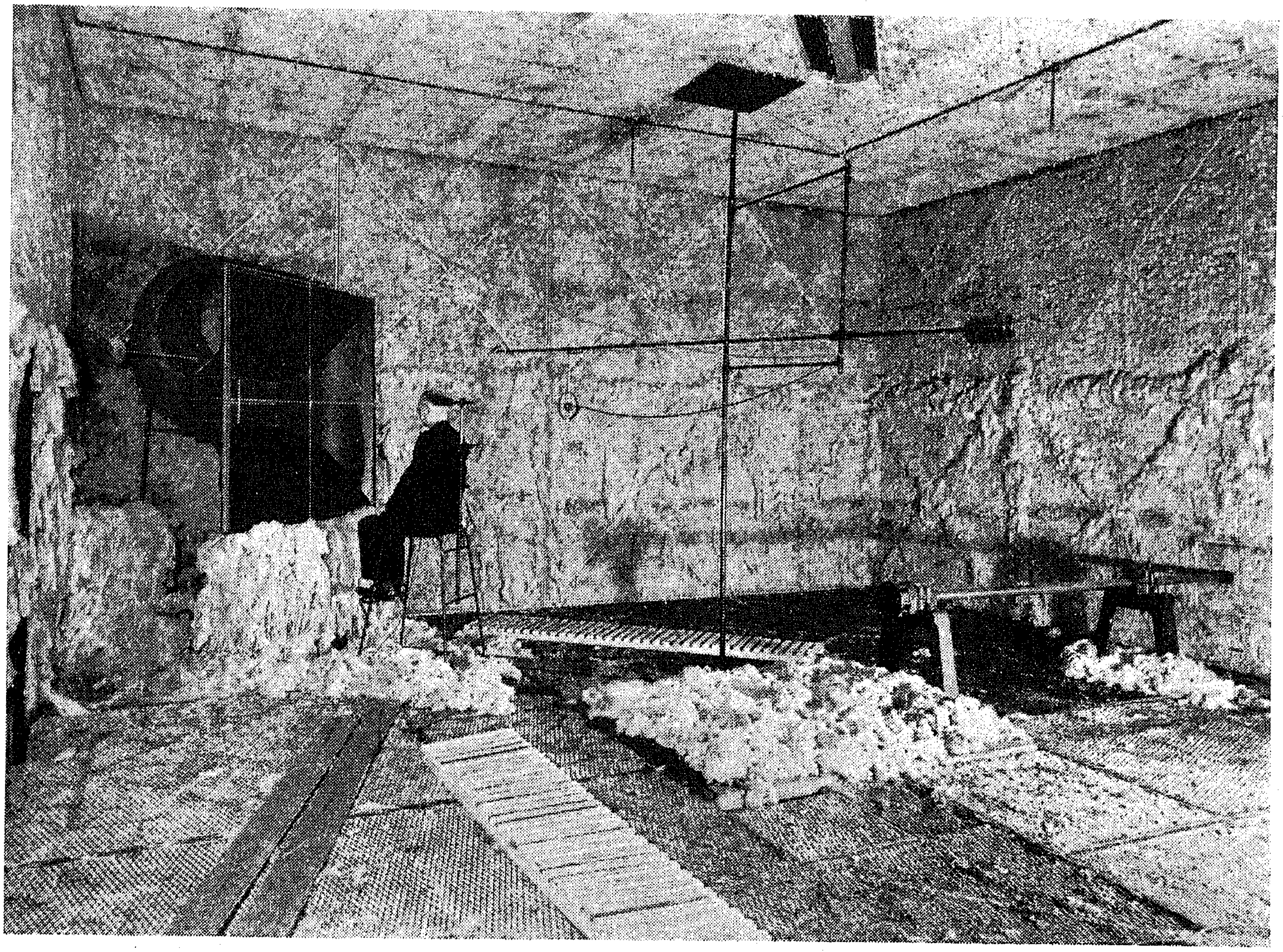

Fig 1B.—Test room for free-field E.L. measurements.

to the sound field is used and normally placed so that the centre of the observer's head is 1 m. from the mouth of the source. Where the highest intensities are required, a distance of 0·5 m. may be used. By means of a swinging arm, a microphone, calibrated in absolute units, can be brought to the position corresponding to the centre of an observer's head so that the frequency, intensity, and purity, of the tones set up may be referred to that point. The basis of the physical measurements is given in Appendix I. Fig. 1B shows a general view of the test room with source C in use.

Three sources are available. Those designated A and C are of the folded horn type, C being capable of high intensities. B consists of a conical-diaphragm loudspeaker without horn. Fig. 2A shows the field distribution with source C. Considering first the attenuation of the reference tone with distance from the mouth of the source, it will be seen that at 1 m. the local deviations from a mean inverse curve do not exceed 0·5 db. (decibels), so that errors due to the exact relative positions of observer's head and microphone in that direction are small. The rate of attenuation of the field, deduced from the mean curve, indicates that the distance from observer to the virtual source is equivalent to over 2 m., so that the plane-wave condition is closely realized, since the distance between ears does not normally exceed 20 cm. The upper diagram shows the horizontal and vertical variation in intensity at the normal location for the reference tone. It is seen that the variations over the region corresponding to the dimensions of the observer's head, e.g. ± 10 cm. horizontally, are negligible. An attenuation/distance curve is also given for 120 cycles. The local irregularities in the region occupied by the observer are quite negligible, and the rate of attenuation indicates a sufficiently close approximation to a plane wave. At 1 m. the horizontal intensity variation does not exceed 0·3 db. for 10 cm. on either side of the central position, and the vertical intensity variation is of the order of 0·2 db. for ± 10 cm. At the 0·5-m. position the variation is even smaller.

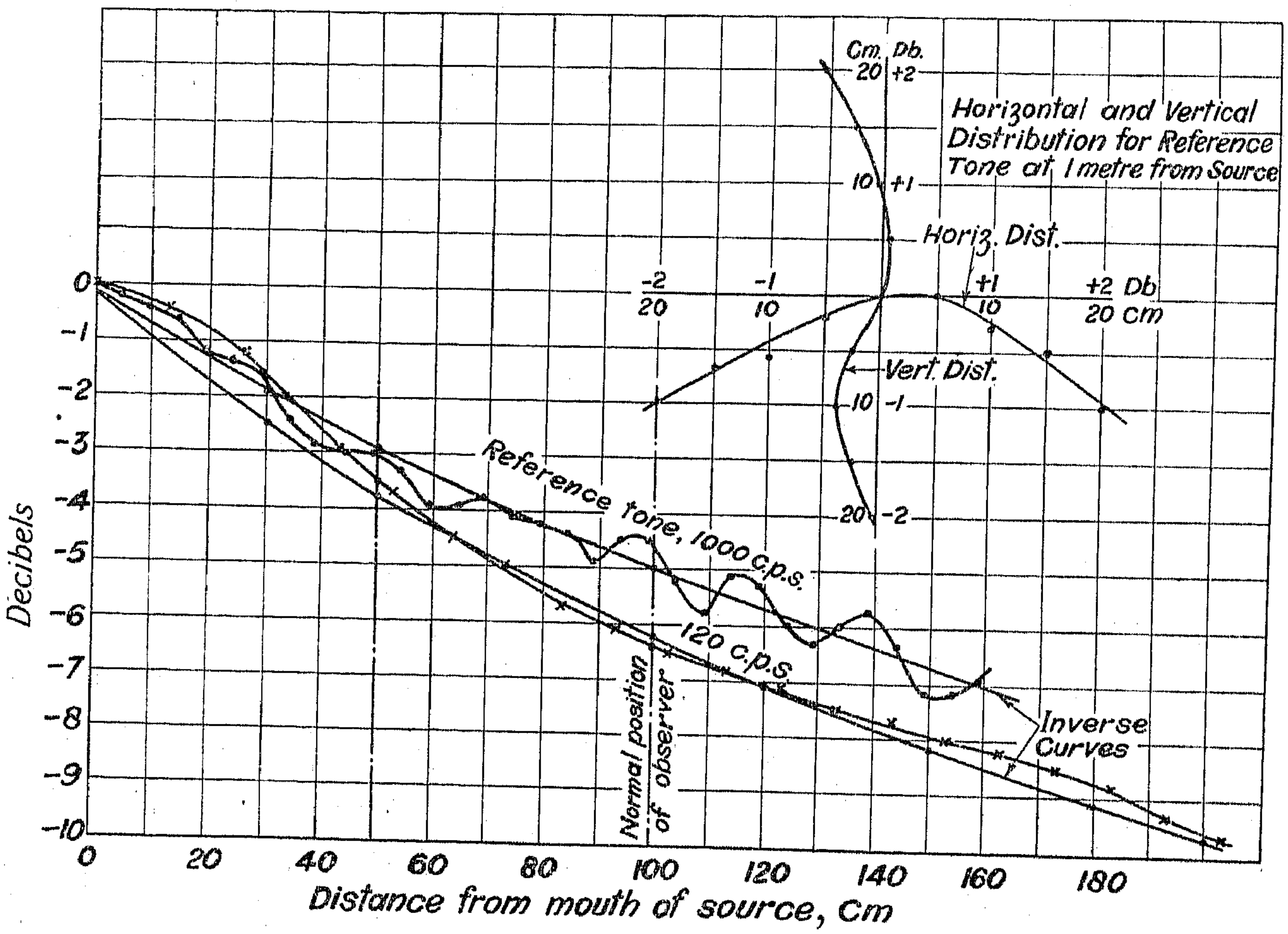

Fig. 2A.—Field distribution in test room. Source C. In absence of observer.

At high frequencies source B is generally used. The local irregularities along a line joining observer and source are satisfactorily small, being not greater than 0·4 db. At 1 m the horizontal and vertical variations for ± 10 cm. from the central position do not exceed 1 db. During the present work the room is cleared of all but essential objects. The effect of the presence of the latter on the field strength at 1 m., central position, was checked. A plank placed on the floor and used for access to the source produced no detectable effect. The reference-tone intensity is, for convenience, measured with the observer's chair in position. A seated observer screens practically the whole of the chair. The effect of the presence of the chair was therefore measured and was found to cause a change of 0·3 db. in the reference-tone intensity at the central position. A head-rest fitted to the chair is sometimes used. Its presence disturbs the field to the extent of 0·2 db. at 1 000 cycles, but more at high frequencies. Since the rest is completely screened by the head when an observer is present, it is removed for field measurements.

A possible source of indefiniteness which is not covered by field measurements is that loud-speakers of different forms may give rise to different multiple-reflection effects between source and observer, particularly when the observer is close to the source. A full examination of this effect would require exploration of the field between subject and loud-speaker. However, a useful criterion of field normality with observer present might be the ratio of the ear-canal pressure to the field pressure at the same point in the absence of the observer, since the loudness sensation experienced is substantially determined by the pressure at the ear canal. This ratio can be determined by a suitable probe-tube technique and, using a group of 5 subjects, satisfactorily reproducible values can be obtained. Some measurements were made with and without a wooden baffle fitted to source B. It was found that, while the presence of the baffle increased the field pressure, the ear-canal pressure was increased correspondingly, so that the ratio of the pressures was unaltered. It was therefore concluded that multiple-reflection effects were negligible, so that the field-pressure measurements described above were sufficient to define the field conditions. It may be noted that for source C the ratio of canal to field pressure at 1 m. is 1·2 db. for 1 000 cycles and 0·0 db. for 120 cycles.

In carrying out E.L. measurements, N and R are checked physically before and after each test. To detect accidental variation of N or R during a test, the operator occasionally notes the readings of a voltmeter connected across the loud-speaker terminals.

(c) The Groups of Observers.

In forming a group of observers for basic measurements it is advisable to exclude persons who, from common knowledge, are known to have defective hearing. In a very few instances, persons are met with who, from nervousness or other reason, are temperamentally unsuitable. Such persons cannot be considered normal observers, and their inclusion merely increases the dispersion of the judgments. Otherwise no discrimination is necessary in selecting observers beyond making the group as representative as may be.

The number of observers required for a group depends on the precision desired in the measurements. It can be shown* that if n is the number of persons in the group and S the standard deviation obtained in a particular measurement, then, provided the distribution of the judgments is normal, the probable error, Y, of the mean in relation to the true value is given by

$$Y = 0{\cdot}67 \frac{S}{\sqrt{(n-1)}}$$

This is the probable magnitude of the error present, but the error may chance to be larger. It can be shown that the probability of the error in a given experiment being $2Y$ is 1 in 5, and of being $3Y$ is 1 in 22. As will be seen later, values of S as great as 4·5 are reached only in a minority of cases. With 10 observers and this value of S the probable error is 1 phon, and with the more normal S value of 3·5 the probable error is well within 1 phon. For most purposes a group of 10 observers is therefore considered adequate. In cases where it is important to verify a measurement, a further measurement with another group of 10 persons affords the best safeguard, as will be shown.

The three groups employed are given in Table 1.

(d) Specimen E.L. measurement.

An example of a typical measurement is the determination of the intensity level of a 360-cycle tone which has an E.L. of 90 phons. Here the usual procedure is reversed; R is held constant at 90 phons, and the intensity-level of the 360-cycle tone (N) is varied.

Theoretically, observations should be taken with both orders of presentation at every 1-db. step for each observer over the entire range in which any want of unanimity is shown—in this case 86·5 to 101·5 db. In practice 15 to 20 observations per observer are found sufficient to define his loudness-equality judgment. The observations are plotted in Fig. 2B. Two dots at a given level indicate a check observation. It will be seen that some of the judgments are quite decisive, while others are contradictory over a range of steps, as shown by the shaded areas. Individual spreads as large as 8 db. are seldom met with, but one happened to occur in the present test. From the plotted judgments Table 2 has been drawn up; this gives the mean votes of the group at each intensity level.

From the mean votes at each level, the group votes for equality at the corresponding levels are obtained, from which the mean intensity level has been calculated. The distribution curve of group votes for equality is shown in Fig. 3. The peak value of this curve gives the modal value of the intensity level, which, in this example, is equal to the mean value, viz. 94·5 db. If the distribution curve is normal, the standard deviation is given by the difference between the mean value and the level at which the second differential of the distribution curve is zero, the slope there being a maximum. The standard deviation is obtained arithmetically by multiplying the square of the deviation from the mean at each level by the number of votes for equality cast at that level. These products are then added and divided by the total number of votes for equality. The square root of this quantity is the standard deviation which, in the present example, is 3·0. This quantity is based on all the available judgments and assumes that all votes are of equal importance, whether they are given by the same or different observers. We may call it the standard deviation of the group equality votes (S_2), as deduced from judgments of loudness predominance.

On the other hand, there is much to be said for the view that the standard deviation should be based on the

Table 1

Group No.	Type of group	Number of observers	Type of observer
I*a*	Technical	5	Men, experienced in acoustical measurement
I*b*	Technical	5	Men, experienced in physical but not acoustical measurements
I	Technical	10	Groups I*a* and I*b* combined
II	Non-technical	10	5 men, 5 women
III	Non-technical	10	5 men, 5 women

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* Yule: "Theory of Statistics."

dispersion of the equality judgments of the several observers rather than on the distribution of the mean votes of the group. When the judgments are decisive, there is no difficulty in proceeding on these lines. The deviation of each observer's equality judgment from the mean value is squared and the mean of the squared deviations taken. The square root of this quantity gives the standard deviation for the individual equality judgments (S_3). In cases where the judgments of an individual observer show a spread, an assumption is necessary regarding the position in the band at which his equality judgment may be presumed to lie. Where there is no inconsistency within the band (e.g. Fig. 2B, Observer A. S. E., where the votes for N and R are equal) it is a fair assumption that the level for loudness equality for that observer corresponds to the centre of the band. However, when judgments within a band are inconsistent (e.g. observer J. W. S.) the course to be adopted is not so obvious. However, adopting the centre-of-band convention for all observers, the standard deviation in the present example is 3·0, i.e. equal to the value obtained for S_2. S_3 is obtained by treating all equality judgments as though they were decisive. Hence the quantity derived is not indicative of the dispersions in the judgments of individual observers. These dispersions must increase the uncertainty of the final result and ought, strictly, to be taken into account. A comparison of the two bases has been made on 22 tests, some on pure tones and others on complex sounds. It is found that on the average the S_2 basis gives a value 11 per cent less than the S_3 basis. The S_3 basis has been used, since it gives, on the whole, a rather more conservative estimate of the degree of certainty of the mean E.L. values. No doubt further investigation would enable a measure of dispersion to be elaborated which would take due account of all the factors involved, but it must be borne in mind that refined methods are only justified when a large number of observations is available. A reasonably simple method of indicating approximately the relative uncertainties of different equivalent loudness determinations is the chief need. A probable-error value relates only to the amount by which the mean value probably deviates from the value for the "normal observer," as inferred from the dispersion of the judgments of the finite number of observers employed. The group itself is assumed to be representative. The probable error cannot take account of factors which would influence all observers similarly, such as the employment of a group which, on account of age or other reason, is unrepresentative or non-compliance with the prescribed physical conditions, e.g. in respect of tone purity. With small groups the standard deviation values on which the probable-error values are based are themselves subject to uncertainty so far as they arise from casual, as distinct from systematic, divergences in the judgments. Thus if a test with 10 observers be repeated, it is unlikely that precisely the same standard deviation value will be obtained, so isolated S.D. (standard deviation) values cannot be regarded as rigidly applicable to the particular conditions of the test. But as the size of the group is increased the S.D. values become more significant and the effect of both casual and systematic divergences on the mean value is diminished. Even with a group of moderate size, the casual element may be almost eliminated if S.D. values from several tests under the same conditions are available, and under suitable circumstances the average S.D. value becomes significant for those conditions.

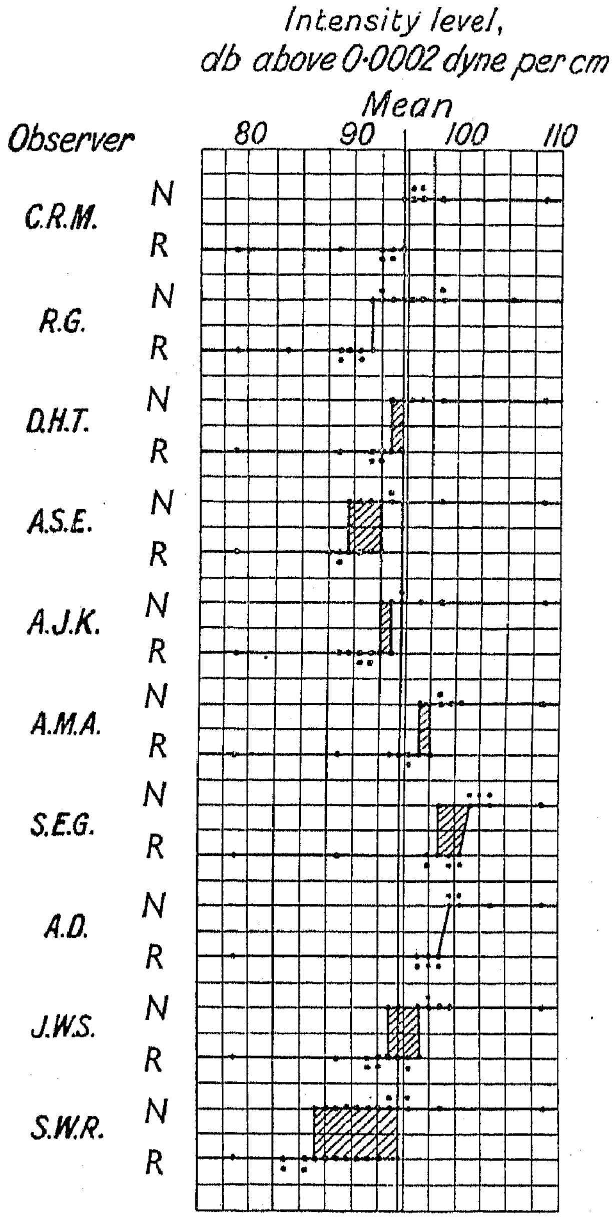

Fig. 2B.—Comparison of 360- and 1 000-cycle tones. Test No. 22; level = 90 phons; group I.

N = 360-cycle tone louder.
R = reference tone louder.

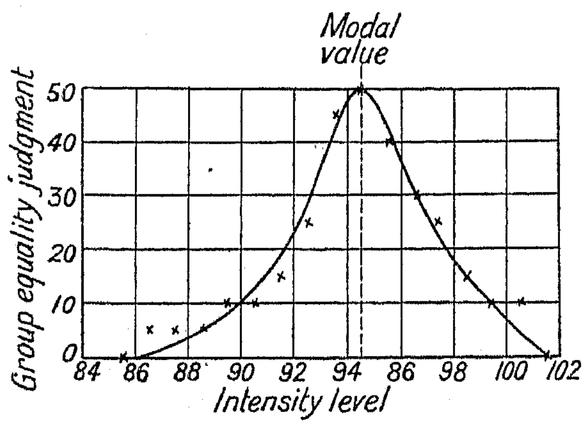

Fig. 3

In a subjective measurement which originates in a complete definition, there is no direct means of determining the error actually present. In the case of E.L., provided the prescribed conditions have been complied with, the mean value is the value according to the definition, to the degree of approximation rendered probable by the dispersion of the judgments. In the calibration of secondary meters, conditions are different. Provided the error of the primary method is negligible, the actual error of the meter is the value it indicates less the basic value.

(4) THE EQUAL-LOUDNESS RELATIONS

Many problems in both the study and application of acoustics require a knowledge of the relative intensity levels of pure tones of differing frequency which sound equally loud. For example, these equal-loudness relations underlie the study of audition and, in particular, the response of the ear to noises of various tone structures. They are essential to the interpretation of analyses of machinery or other noise, their use enabling the relative importance of the constituent tones to be assessed. They form a basis on which the response of objective meters may be adjusted, and will play an essential part in the future development of such meters.

Table 2

SUMMARY OF JUDGMENTS. (Test No. 25)

Observer	Intensity levels (db. above 0·0002 dyne per cm^2)																
	85·5	86·5	87·5	88·5	89·5	90·5	91·5	92·5	93·5	94·5	95·5	96·5	97·5	98·5	99·5	100·5	101·5
	Percentage votes for N being the louder.																
C. R. M... ..	0	0	0	0	0	0	0	0	0	50	100	100	100	100	100	100	100
R. G.	0	0	0	0	0	0	50	100	100	100	100	100	100	100	100	100	100
D. H. T... ..	0	0	0	0	0	0	0	0	50	50	100	100	100	100	100	100	100
A. S. E.	0	0	0	0	50	50	50	50	100	100	100	100	100	100	100	100	100
A. J. K.	0	0	0	0	0	0	0	50	50	100	100	100	100	100	100	100	100
A. M. A... ..	0	0	0	0	0	0	0	0	0	0	0	50	50	100	100	100	100
S. E. G.	0	0	0	0	0	0	0	0	0	0	0	0	0	50	0	0	100
A. D.	0	0	0	0	0	0	0	0	0	0	0	0	0	0	100	100	100
J. W. S.	0	0	0	0	0	0	0	0	50	50	0	50	100	100	100	100	100
S. W. R... ..	0	50	50	50	50	50	50	50	50	100	100	100	100	100	100	100	100
Mean votes for N being the louder	0	5	5	5	10	10	15	25	45	50	60	70	75	85	90	90	100
Group equality votes	0	5	5	5	10	10	15	25	45	50	40	30	25	15	10	10	0

Mean intensity level = 94·5 db.
Modal value (from Fig. 3) = 94·5 db.

Wegel* and Kingsbury† were early investigators of these relations. The present authors, following Kingsbury, made some supplementary determinations.‡

The most important contribution to the subject consists of the comprehensive data of Fletcher and Munson,§ which were determined with a greatly improved technique. The data were obtained by means of telephone receivers calibrated at each frequency in terms of a free field of the same frequency, the value of which was known in absolute measure. In the loudness comparisons a tone of a given frequency and intensity level, alternately with a 1 000-cycle tone, was presented to an observer's ears by means of the calibrated telephones, and the equal-loudness conditions were deduced, using the C.S. procedure described in Section I(*a*). Although for ordinary noise problems it is only the free-field condition which is strictly relevant, it is to be expected that relations determined through the medium of calibrated receivers will be generally similar to those determined directly in the free field. How far equal loudness relations obtained with a comparatively limited number of observers may be considered generally applicable is a point on which little information has been available. Some additional values determined directly in the free field and necessarily with other observers were therefore thought to be of interest.

Equal-loudness values are referred to 1 000 cycles and 0·0002 dyne per cm^2 reference pressure. The determination of free-field equal-loudness relations therefore consists of measuring the E.L. of pure tones of given frequencies and intensity levels, the technique for which has already been described. The values obtained are given in Table 3. The second column gives the frequency and the third the E.L. level at which a determination was made. The fourth column indicates the source used. The fifth gives the purity of a tone of the given frequency in terms of the difference in E.L. between the largest harmonic or other parasitic tone and the fundamental. The present equal-loudness relations were used in computing this. Tone purity is especially important when comparing single tones, and is difficult to attain at low frequencies and high inten-

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* R. L. WEGEL: *Bell System Technical Journal*, 1922, vol. 1, p. 56.

† B. A. KINGSBURY: *Physical Review*, 1927, vol. 29, p. 588.

‡ B. G. CHURCHER, A. J. KING, and H. DAVIES: *Journal I.E.E.*, 1934, vol. 75, p. 401.

§ *Loc. cit.*

Table 3

Equal-Loudness Determinations

(Cases where parasitic tones are below threshold are denoted by X.)

No.	Frequency	B.S. phons	Source	Purity	Group	Intensity Levels		Standard deviation	Probable error	Final value adopted
						Mode	Mean			
1	54	87	C	32	I	94	94	2·0	0·5	
	54	90								96
	54	80								88
	54	60								78
2	54	50·5	C	36	I	73	73·5	10·4	2·3	
	54	40								69
3	54	10	C	X	I	54·5	54·5	3·6	0·8	54·5
4	54	0	C	X	I	—	38·5	4·4	1·0	—
5	60	80	B	20	I	86·5	87	3·4	0·8	87
6	60	60	B	25	I	77·5	77	2·9	0·7	77
7	60	40	B	30	I	65·5	68	4·6	1·0	68
8	60	10	B	X	I	54·5	53·5	3·0	0·7	53·5
9	120	90	A	25	I	99·5	98	4·5	1·0	—
10	120	90	C	35	I	95·5	96	3·0	0·7	97
11	120	80	A	28	I	86	85·5	3·5	0·8	—
12	120	80	B	39	I	90·5	90	2·3	0·5	—
13	120	80	B	39	II	91	91·5	2·6	0·6	—
14	120	80	B	39	III	88·5	89·5	3·4	0·8	—
15	120	80	C	45	I	89	89	2·7	0·6	89
16	120	60	C	40	I	74	73	4·0	0·9	73
17	120	40	A	35	I	57·5	58	2·2	0·5	58
18	120	10	A	X	I	35	35	3·4	0·8	—
19	120	10	A	X	II	37·5	35	3·9	0·9	—
20	120	10	A	X	III	37	36·5	2·8	0·6	—
21	120	10	C	X	I	36·5	36·5	4·2	0·9	36
22	120	0					27			27
23	200	90	A	30	I	96·5	96·5	2·2	0·5	96·5
24	200	80	A	32	I	85	85	2·8	0·6	85
25	360	90	A	40	I	94·5	94·5	3·0	0·7	—
26	360	90	C	40	I	96	96	2·6	0·6	95
27	360	80	A	40	I	82	81	2·3	0·5	—
28	360	80	C	40	I	83	83	3·2	0·7	82
29	360	60	A	40	I	62	62	2·3	0·5	62
30	360	40	A	40	I	43	43·5	3·8	0·9	43·5
31	360	10	A	40	I	16·5	16·5	3·2	0·7	16·5
32	360	0				—	7·5			
33	1 000	0	C	X	I	− 1·5	− 2	3·5	0·8	− 2
34	1 600	80	B	40	I*a*	79·5	78·5	2·6	0·9	78·5
35	3 200	90	B	40	I	82	83	5·7	1·3	85
36	3 200	80	B	40	I	72	73	3·1	0·7	—
37	3 200	80	B	40	I	76	76	5·0	1·1	75
38	3 200	80	B	40	II	78	78·5	3·4	0·8	—
39	3 200	60	B	40	I	58	58	5·1	1·1	55
40	3 200	40	B	40	I	35	35	5·3	1·2	35
41	3 200	10	B	40	I	5	5	6·9	1·5	
42	3 200	10	B	40	II	4	4·5	4·6	1·0	5
43	3 200	0	—	40			0			
44	4 500	80	B	40	I	78	78	3·9	0·9	78
45	6 400	80	B	40	I	86	85	5·6	1·3	87
46	6 400	60	B	40	I	71	69	6·4	1·4	69
47	6 400	40	B	40	I	54	54	4·9	1·1	51
48	6 400	10	B	40	I	23	22	7·5	1·7	24
49	6 400	0	—				15			
50	9 000	60	B	40	I	70	70	6·5	1·5	70
51	9 000	40	B	40	I	52	51	9·0	2·0	52
52	9 000	10	B	40	I	28	27	8·3	1·9	27

sities. The basis of Fletcher and Munson's theory of the response of the ear to complex sounds* is that the loudnesses of the several components are additive, allowance being made for masking. This interesting basis may be used to illustrate the order of purity advisable in basic E.L. measurements. Consider the case of a 1 000-cycle pure tone to which a harmonic is added. Neglecting masking as unimportant on account of the considerable frequency separation, the E.L. of the impure tone, and hence the error in an E.L. measurement due to the impurity, may be computed. The phon-loudness relation of Fig. 3 (Ref. 2) has been used for this purpose. To take an extreme example, a harmonic of 80 phons added to a fundamental of 80 phons would give a total E.L. of 91 phons, i.e. an error of 11 phons. If the harmonic be reduced by 10 phons, the error would be about 6·5 phons. The present authors conclude that it is necessary, in general, to keep the largest harmonic 30, or if possible 40, phons below the fundamental. (It may be noted that the E.L. values for noises A, B, C, and D, in Table 4, are concordant with the loudness-summation basis. The equivalent energy-summation basis for computing the combined effect of tones, according to which a harmonic of 80 phons added to a fundamental of 80 phons should give a resulting E.L. of 83 phons, is clearly not relevant.) In addition to harmonics, the possibility of the presence of "mush," which is not readily detectable by sound analysis, was given critical attention. In the measurements recorded in Table 3 the reference-tone purity was always better than 35 phons below fundamental. Col. 6 gives the group of observers employed. Modal and mean intensity levels to the nearest 0·5 phon are given in the next two columns, followed by the standard deviation and estimated probable error of the mean value. The last column gives the value finally adopted for the equal-loudness relations.

Considering first the frequencies below 1 000, it is seen that modal and mean values differ in two cases only by as much as 2·5 db., and agree on the whole to 1 db., indicating that the distribution curves of the judgments are substantially symmetrical. As Group I was the principal group used throughout the determinations, tests 12, 13, and 14, at 120 cycles and 80 phons were carried out to ascertain how far this group might be considered representative of the "normal observer." It is seen that the three groups give intensity levels differing by not more than 1 db. from the mean, which is 90·5 db., which differences are consistent with the estimated probable errors.

Similarly tests 18, 19, and 20, at 120 cycles and 10 phons with the three groups give good agreement, the differences from the mean of 35·5 db. not exceeding 1 db. Hence it may reasonably be concluded that in this frequency region Group I is adequately representative of the "normal observer." While tests have been generally made at the exact phon value desired, this has not always been experimentally convenient, so that intermediate values have been obtained by interpolation from curves of phons and intensity level for a given frequency. A family of such curves has been plotted for all frequencies examined and has been used as a guide in selecting the final values adopted. But, as seen from the Table, in most cases the observed values have been adopted directly. At the low frequency of 54 cycles per sec. the tone required a noticeable time to build up to full value. In order that its loudness might be adequately judged, a time of presentation of 3 seconds was employed for this frequency.

In test 1 the maximum intensity consistent with adequate tone purity was set up. A threshold determination* was attempted at this frequency but some observers reported that they were of opinion that the sensation experienced was one of feeling rather than hearing. The intensity level obtained was so inconsistent with the other data that, although it is recorded, it has not been adopted. The low value obtained in test 11 compared with tests 12 to 14 is almost certainly due to inadequate tone purity. The agreement between tests 12, 13, and 14, from a subjective standpoint has been noted, but the result of test 15 has been adopted owing to the better field and purity conditions.

The results are plotted in Fig. 4, Fletcher and Munson's contours being shown dotted. The good agreement at the 60-phon contour is to be noted. The calibration of the telephone receivers on which Fletcher and Munson's determinations were based was carried out for different frequencies at intensity levels within a few decibels of those corresponding to the 60-phon contour, so that here any possible difference between telephone and free-field conditions may be expected to be a minimum.

As we proceed to the 40- and 10-phon levels for frequencies below 1 000, the points are seen to diverge from the dotted curves in an increasing and regular manner. The threshold values plotted, some of which are included in Table 3, were obtained with various groups of observers on various occasions and are seen to give a curve consistent with the 10-phon points. They are also in good agreement at the lower frequencies with free-field threshold values published by Sivian and White.†

At the 80- and 90-phon levels the divergence is regular but of opposite sign. To verify this, additional points were taken at 200 cycles; these are seen to be concordant with those at 120 and 360 cycles.

From Table 3 the probable error of the mean (except in Test 2) is 1 db. or less. Hence individual points may possibly be in error by 2 or even 3 db., but where the present values diverge from the telephone values they do so in a regular manner not suggestive of fortuitous errors of several db. in the individual points. Hence systematic divergences of 2 db. or more may be regarded as significant. In addition to the above subjective errors, there are the errors in the absolute physical magnitudes. In this frequency region an error approaching 1 db. (or 12 per cent) in acoustical pressure is very improbable, and an error of constant amount would not explain divergences of opposite sign at high and low levels. The practice of immediately checking the apparatus at every frequency and level at which a test is made is thought to guard against errors in the pressure measurements comparable with the subjective errors.

There may be several causes for the increased intensity levels required with telephone receivers for a given E.L. at low levels and frequencies. The pressure of the receivers on the head constitutes an additional stimulus differing from that of the acoustic pressure, the effect of the added stimulus apparently becoming progressively less important as the intensity level is increased. The presence of the receivers may also alter the motional impedance of the ear-drum and may also have some purely psychological effect. The sound heard with telephones differs in nature from a corresponding free-field sound. In the telephone determinations calibrations were carried out near threshold level as well as at 60 phons, but reliance was placed on the latter. Corrections relative to 1 000 cycles may be deduced, which are + 2·6, — 4·3, — 2·6, and + 0·4 db., at 60, 120, 240, and 480 cycles, respectively. These corrections go a considerable way towards bringing telephone and free-field threshold data into agreement at 120 and 240 cycles.

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* *Loc. cit.*

* See Test No. 4 in Table 3.

† L. J. Sivian and S. D. White: *Journal of the Acoustical Society of America*, 1933, vol. 4, p. 288.

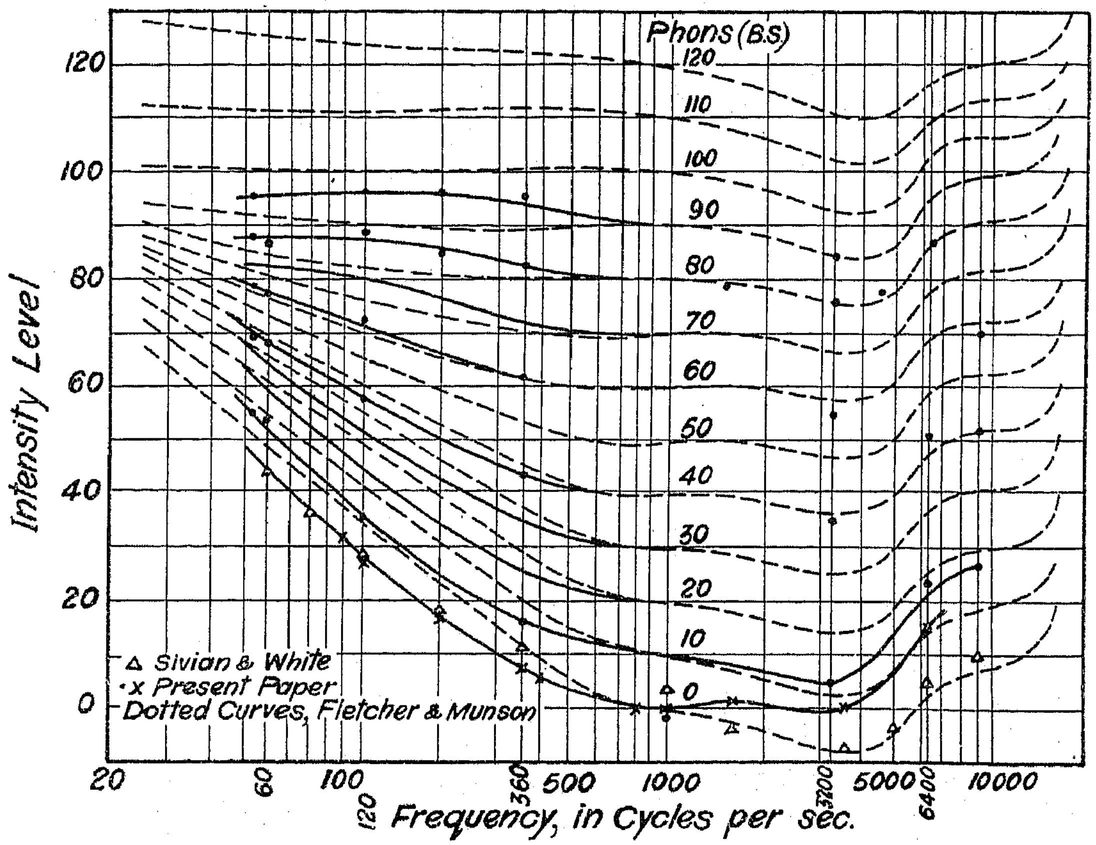

Fig. 4.—Equal-loudness relations.

The divergence between telephone and free-field values at high levels would have to be of opposite sign if impurity in the free-field tones were to account for it.

Considering now the values for frequencies above 1 000, increased uncertainty is to be expected. The criterion of equal loudness seems less definite than at low frequencies, the high-frequency sound giving a thin piercing sensation rather than the sensation of "volume" evoked by low-frequency sounds. The sound fields were not so uniform as at low frequencies, so that the effect of the position of the observer's head was rather more critical. In particular, the inclination of the observer's head was found to be very important, so its location and inclination were made more definite by adopting a sighting check. For this, the observer was asked to keep his head in such a position that, with the left eye closed, he sighted two points in a straight line.

These factors are reflected in the increased dispersion, which gives rise to probable errors up to 2 db. Tests 36 and 37 with Group I were separated by an interval of 3 weeks and illustrate the reproducibility of determinations in this region. Group II gave a result differing from the mean of these two by 4 db. Tests were also carried out at 3 200 cycles and 10 phons with Groups I and II (tests 41 and 42) and, in view of the estimated probable errors, the close agreement between the two is fortuitous. Referring to Fig. 4, the agreement between telephone and free-field values for the 60-phon contour has already been mentioned. Four points on the 80-phon contour and one on the 90 also show good agreement.

The 10-phon points are consistent in themselves and not inconsistent with the authors' threshold values. J. C. Steinberg* has shown that loss of hearing with age is negligible for frequencies up to 1 000, and small up to 2 000. At 4 000 and 8 000 cycles it becomes marked, the loss of hearing for persons between 40 and 49 years of age being of the order of 18 db., compared with those between 20 and 29 years old. Hence the divergences between the 10-phon and between the zero-phon contours at high frequencies may well be due to a difference in the average age of the observers employed in the two investigations, and unless observers of a specified range of age were employed the high-frequency contours are not closely definable at low levels.

Such high-frequency values as have been obtained confirm the marked inflections in the contours. From other evidence which cannot be discussed here they appear to be largely accounted for by the variation of the ratio of ear-canal pressure to field pressure with frequency. This ratio varies not only with frequency but considerably from one observer to another, a factor which probably contributes to the larger dispersion at high frequencies. Some further high-frequency determinations under improved experimental conditions are under consideration.*

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* *Bell Telephone Laboratory Quarterly*, January, 1936.

Whether the data should be expressed in the form of modal or mean values is doubtful. The modal value does not attach so much weight to occasional dissentient judgments, whereas the mean assigns equal importance to all judgments. In the present data there is no systematic or important difference between the two. Considering the difficulties of high-frequency tone comparisons, the authors regard such differences as appear at frequencies over 1 000 between the telephone and free-field measurements as being due to experimental error, an additional possible cause of divergence at low levels being the average age of the observers. At frequencies below 1 000, the probability of error is much smaller and the systematic divergence of free-field and telephone data appears to indicate that the two methods do not lead to the same result in this region. At 120 cycles the intensity-level interval corresponding to a change of E.L. of 0 to 90 phons is 56 db. for the telephone values and 70 for the directly-determined values. The 10-phon contour is of technical interest and may occasionally be needed in practical noise problems connected with very small domestic apparatus. The 80- and 90-phon contours are probably the two most important for machinery-noise problems.

It would have been of interest to have determined the 100-phon contour,* but such loud sounds are trying to the observers and free-field tones of such intensities and of adequate purity are exceedingly difficult to produce at low frequencies. High-intensity low-frequency pure tones can be produced with certain types of telephone receiver, but it is felt that the natural mode of listening must be regarded as the final criterion.

The free-field contours for 20 and 30 phons have been deduced by interpolation from curves relating E.L. and intensity level for a given frequency.

The comparison of the two sets of data, obtained from independent investigations and with different observers, indicates that the equal-loudness relations are definable in terms of the "normal observer" with a precision that is adequate for present needs.

(5) THE EQUIVALENT LOUDNESS OF CERTAIN PHYSICALLY SPECIFIED NOISES

In Table 4 are given E.L. values determined for certain physically specified noises. The compositions of the noises were chosen in some cases to demonstrate characteristics peculiar to certain noise meters, and in other cases to be broadly typical of certain industrial noises. Two general points may first be noted. Tests 57, 58, and 59, were carried out to verify the effect of the time of presentation of the sounds. The times were 2 seconds (normal), 5, and 10 seconds, and it is seen that there is no significant difference in the E.L. values obtained, showing that the presentation time is not critical. Tests 61 and 62 were carried out with the technical and one of the non-technical groups respectively. Such close agreement of either group with the combined group of 20 observers is, of course, fortuitous, but it confirms tests 12, 13, 18, and 19, of Table 3, in showing that the E.L. values obtained by the two groups do not differ materially.

Series of harmonic tones with fundamental frequencies of approximately 100 are of interest in relation to transformers and other power-frequency static electromagnetic apparatus. Harmonic series, with non-harmonic components in addition, appear in dynamo-electric machines and also in mechanical plant, especially where gearing is involved. Harmonic series of relatively high frequencies occur in fan noise. Hence measurements have been made on harmonic noises having fundamental frequencies of 120 cycles per sec. and equally loud components, and also on some with unequally loud components. Non-harmonic noises having lowest frequencies of 120 and 480 cycles have also been included.

Noise P contains both harmonic and non-harmonic components covering a frequency range of 120 to 5 900 cycles per sec. and is typical of a geared turbo-generator set. Q is a non-harmonic high-frequency noise of high intensity. R, S, and T, are impulsive noises, such as arise with internal-combustion engines and some other types of plant. Tests 60 and 66 and also 64 and 67 show no consistent difference in the E.L. of harmonic and non-harmonic noises of similar composition at moderate levels. If a difference occurs at all, it is likely to occur at very high intensities where subjective tones become large.

(6) THE PERFORMANCE OF SECONDARY NOISE METERS

(a) Subjective meters. General.

The portable subjective noise meter is well known and takes many forms. In one, formerly known as the "audiometer," the observer listens simultaneously to the noise with one ear and a reference tone of adjustable intensity presented to the other ear by a telephone receiver. He then adjusts the reference tone by trial to what he judges to be loudness equality with the noise. Both complex and pure reference tones have been used in this way. (In another form of instrument, known as the "spaced telephone" type, the observer listens simultaneously to noise and reference tone with both ears, the reference tone being presented by a pair of receivers spaced a short distance from the ears. The observer then adjusts the reference tone until it is just inaudible in the presence of the noise. This "masking" value is not related in any simple manner to the E.L. of the noise, and the method will not be further discussed.) The conditions under which these meters operate differ in some important respects from the conditions for basic E.L. measurement. The meters do not, in general, give indications in accordance with the standard. The authors have therefore experimented with a type of subjective meter from which as many of these differences as possible have been removed. This meter and its performance will now be described.

(b) Two-telephone subjective meter.

In this meter alternate listening is made possible by the use of two receivers fitted with soft rubber caps, so that when the receivers are placed on the ears the reference tone is heard and the noise under measurement is substantially excluded. The observer faces the source, and the criterion of loudness equality of noise and reference tone is that when the receivers are quickly removed or applied no change of loudness is detectable. To reach this condition the observer finds attenuator settings which are somewhat too high and somewhat too low for loudness equality and by repeated trial narrows down the interval between the two until he reaches a setting which he judges to give loudness equality. The attenuator is adjustable in 5-db. steps from 0 to 100 db., and is supplemented by a fine attenuator calibrated in 1-db. steps covering a range of 10 db. The frequency of the reference tone is 800 cycles per sec., a value found convenient for the measurement of industrial noise. A test was carried out to ascertain to what degree the noise under measurement is heard when a pair of receivers of the type used is in position on the head. A free-space pure tone of known intensity level and frequency was set up. Arrangements were made by which the observer could switch on this tone or, alternatively, the reference tone set up by the receivers and also adjust the reference-tone intensity.

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* See Appendix III.

Table 4

EQUIVALENT LOUDNESS OF CERTAIN PHYSICALLY SPECIFIED NOISES

(Note: Frequency and equivalent loudness of components are expressed by *f*/E.L.)

Test No.	Noise	Composition of noise	Group	B.S. phons	Standard deviation	Probable error
53	A	Two harmonic tones:—120/50, 240/50	I*a*	61	1·5	0·5
54	B	Two harmonic tones:—120/30, 240/30	I*a*	37·5	3·6	1·2
55	C	Two harmonic tones:—120/50, 720/50	I*a*	59	1·8	0·6
56	D	Two non-harmonic tones:—120/70, 680/70	I*a*	80	0·9	—
57	E	Six harmonic tones:—120/80, 240/80, 360/80, 480/80, 600/80, 720/80 ($T = 2$)	I	91	1·9	0·5
58	E	Ditto. ($T = 5$)	I	91	3·1	0·7
59	E	Ditto. ($T = 10$)	I	90·5	1·3	0·3
60	F	Six harmonic tones:—120/61, 240/61, 360/61, 480/61, 600/61, 720/61	I	79	2·5	0·6
61	G	Six harmonic tones:—120/40, 240/40, 360/40, 480/40, 600/40, 720/40	I	62·5	1·7	0·4
62	G		II	62·5	3·9	0·9
63	G		I, II	62·5	3·6	0·6
64	H	Six harmonic tones:—120/30, 240/30, 360/30, 480/30, 600/30, 720/30	I	53	3·6	0·8
65	I	Six harmonic tones:—120/10, 240/10, 360/10, 480/10, 600/10, 720/10	I	26	4·8	1·1
66	J	Six non-harmonic tones:—120/60, 265/60, 310/60, 420/60, 560/60, 680/60	I	79·5	3·1	0·7
67	K	Six non-harmonic tones:—120/30, 265/30, 310/30, 420/30, 560/30, 680/3	I	52	4·2	1·0
68	L	Six non-harmonic tones:—480/50, 1 080/50, 1 240/50, 1 680/50, 2 200/50, 2 500/50	I	75	2·5	0·6
69	M	Six non-harmonic tones:—480/30, 1 080/30, 1 240/30, 1 680/30, 2 200/30, 2 500/30	I	53	4·1	0·9
70	N	Six harmonic tones:—120/85, 240/86, 360/87, 480/88, 600/89, 720/90	I	95	1·5	0·4
71	O	Six harmonic tones:—120/24, 240/24, 360/23, 480/22, 600/21, 720/20	I	46	3·8	0·9
72	P	Ten tones:—120/60, 240/65, 360/68, 480/62, 600/67, 1 100/56, 2 200/56, 2 500/69, 4 080/58, 5 900/69	I	88	2·3	0·5
73	Q	Five tones:—485/90, 700/90, 1 080/91, 2 200/92, 2 500/93	I	103	3·0	0·7
74	R	Impulsive noise. Saw-tooth wave-form. 50-cycle fundamental	I	91	2·3	0·5
75	S	Impulsive noise. Saw-tooth wave-form. 50-cycle fundamental	I	73	2·7	0·6
76	T	Impulsive noise. Saw-tooth wave-form. 10-cycle fundamental	I	80	2·8	0·6

The observer, facing the source and with the receivers applied to his ears, hears the free-space tone but with diminished loudness owing to the presence of the receivers. In the test he adjusted the reference tone intensity until, with alternate listening, he judged this diminished loudness and that of the reference tone to be equal. This was carried out by the observers of group I*a*. Separate tests were made to calibrate the telephone reference tone in terms of the free-field tone. The following results were obtained. With a free-space tone of 120 cycles, 80 phons, the mean sound attenuation due to the presence of the receivers on the head was 22 db. This is equivalent to an impurity in the reference tone of about 35 phons below fundamental. At this low frequency considerable variation among observers was noted. A test at 2 000 cycles, 80 phons, gave 34 db. attenuation. As the attenuation indicated appeared adequate, tests were next made in which the readings obtained with a two-telephone noise meter were examined for noises of known E.L. The results are shown in Table 5. All the tests except two were taken by group I, consisting of 5 observers accustomed to subjective meter measurements and 5 not accustomed. The instrument adjustment for pure tones was first checked. From Table 5 it is seen that tests 1, 2, and 3, give points showing a divergence between E.L. and meter reading of not more than 2 db. Tests 4 and 5 indicate a marked departure from a linear relation at low levels, which seems confirmed by test 10, noise I. Such an effect is not unexpected with telephones at low levels and is receiving further examination. For many purposes it may be sufficient to assume a linear relation for the meter for levels above 30 phons, i.e. that no correction to the readings is required above that level. The errors given in Table 5 are calculated on that basis. Taking tests 6 to 21, omitting No. 10, the average error of meter reading, allowing for sign, is + 0·7 db. The average error, regardless of sign, is 2·3 db. This is the average amount by which the mean reading of a group was in error if a linear calibration curve for the meter be assumed. The simple expedient of taking the average of the decibel errors at different levels has some justification, since 1 db. represents a constant percentage increment in stimulus at all levels, although it does not represent a constant increment in loudness. The maximum error in any test occurs with noise M, the error being + 5 db.

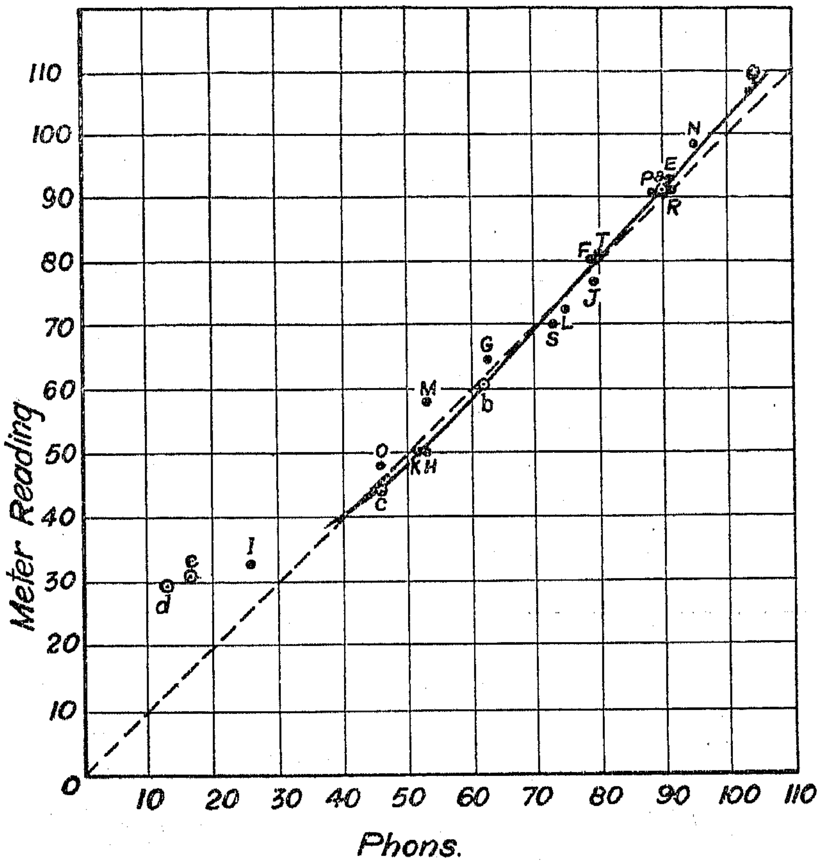

Fig. 5.—Results obtained with two-telephone subjective noise meter.

● ● Synthetic noises. ⊙ ⊙ Pure tones.

On the other hand it is seen from Fig. 5 that a smooth curve of simple form can be drawn through the three pure-tone points. This suggests that the law of the meter is not quite linear at high levels. If we assume that the curve is the law of the meter for any type of noise and correct the readings in accordance with it, we obtain for tests 6 to 21 inclusive an average error of + 0·6 db. The average error, regardless of sign, is 1·7 db., and the maximum error, which again occurs with noise M, is 7·5 db. Thus the curve based on pure tones also fits the noise observations more completely than the linear relation, and as this conclusion is based on 21 tests, none of which is very discordant, it is probable that the curve represents the law of the meter. As to whether corrections in respect of the divergence between the curve and linear relation are worth applying is doubtful. It must be remembered that the mean of a set of 10 readings probably contains a chance error of 2 or 3 db., so that corrections of 2 db. or less may not be worth applying. From Table 5 the average probable error calculated from the standard deviations is 1·3 for tests 6 to 21. This is the average error probably present in the mean meter readings as a whole, as inferred from the dispersion of the observers' judgments. It does not include errors from other sources, such as errors in the basic E.L. values, errors in the physical adjustments of the meter, or such errors as may arise from an assumption that the law of the meter is linear when it is only approximately so. The average amount by which a test is in error on the assumption of a linear law is 2·3, whereas if the law indicated by the curve is assumed the average error is 1·7, which is little greater than the average probable error of the meter readings, viz. 1·3. It is known from pressure measurements that the telephone receivers used are linear up to 110 db. Hence the divergence from the linear relation is to be ascribed to subjective factors.

In subjective secondary measurements a small group of experienced observers will often be employed. It is therefore of interest to compare the values obtained by the two sub-groups, I*a* consisting of five experienced observers and I*b* of five observers not experienced. Taking the values above 30 phons and assuming a law for the meter in accordance with the curve, the average amount by which a test is in error is 2·7 db. for I*a* and 3·1 db. for I*b*. Taking account of sign, group I*a* gives values which are 1·5 db. too high on the whole, whereas group I*b* gives values 1·0 db. too low. A tendency for the readings of beginners to be lower than those of experienced observers has been noticed in one-telephone measurements.

To sum up, the two-telephone meter enables the E.L. of any type of noise above 30 phons to be measured without correction to about 4 phons with a group of five experienced observers, or within about 2·5 phons with 10 observers. If a calibration curve based on pure tones of known E.L. be used, it appears that the average error may be reduced to approximately 2·5 phons for five experienced observers and 2·0 for 10 observers.

Table 5

Two-telephone Subjective Noise Meter

Test No.	Noise	B.S. phons	Two-telephone meter		Error	Standard deviation	Probable error
			Group	Reading (mean)			
1	(*a*) 1 000-cycle tone	90	I	91·5	+ 1·5	3·7	0·9
2	(*b*) 1 000-cycle tone	62	I	60·5	− 1·5	5·5	1·2
3	(*c*) 1 000-cycle tone	46	I	44	− 2	7·0	1·6
4	(*d*) 360-cycle tone	13	I	29	+ 16	4·5	—
5	(*e*) 120-cycle tone	17	I	31	+ 14	5·9	—
6	E	91	I	93	+ 2	3·8	0·9
7	F	79	I	80	+ 1	4·9	1·1
8	G	62·5	I	64·5	+ 2	7·1	1·6
9	H	53	I	50	− 3	8·5	1·9
10	I	26	I	32·5	+ 6·5	6·1	1·3
11	J	79·5	I*a*	77·5	− 2	2·0	0·7
12	K	52	I	50	− 2	7·6	1·7
13	L	75	I	72·5	− 2·5	5·0	1·1
14	M	53	I	58	+ 5	7·6	1·7
15	N	95	I	99	+ 4	3·4	0·8
16	O	46	I	48	+ 2	5·5	1·2
17	P	88	I	90·5	+ 2·5	5·2	1·2
18	Q	103	I	107	+ 4	3·9	0·9
19	R	91	I	91	0	5·9	1·3
20	S	73	I	70	− 3	7·6	1·7
21	T	80	I*a*	81	+ 1	5·4	1·8

Table 6

No.	Nature of plant	No. of observers	B.S. phons	Standard deviation
1	75 000-kvA transformer	4	70	3·3
2	75 000-kvA transformer	4	61	2·0
3	75 000-kvA transformer	6	85	5·8
4	75 000-kvA transformer	6	77·5	2·0
5	75 000-kvA transformer	6	72	2·0
6	12 500-kvA transformer	5	68·5	3·0
7	12 500-kvA transformer	5	77	5·2

Table 6 gives some values obtained with experienced observers on engineering plant with the two-telephone meter. These values will be referred to later in connection with the performance of the objective meter.

The different tests on the same apparatus refer to different conditions of operation, which are not material to the present discussion. With small numbers of observers individual standard deviation values are not highly significant, but the average figure of 3·3 is similar to that obtained by the same observers under laboratory conditions.

The degree to which two-telephone measurements by a group of 5 practised observers could be relied upon was explored in more detail in the following test. Noise G was set up. From Table 4 its value, as determined by groups I and II (20 observers), is seen to be 62·5 phons, with a probable error of 0·6. The noise was then measured by a two-telephone meter, the observers being those of group I*a*. On this occasion the attenuator dial was covered, so that an observer could not see his own settings. The voltmeter embodied in the meter, by means of which the operating voltage is set, was also covered. Four sets of observations were carried out, two in the morning and two in the afternoon of the same day. All readings were recorded by an experimenter, so that the observers did not know their own settings or those of other observers. The experimenter also varied the voltmeter setting and hence the zero of the meter scale over a range of 7 db. without the knowledge of the observers. The corrected readings are given in Table 7.

The readings of some observers throughout the four tests are seen to be more consistent than those of others. By taking the average for the four tests we almost eliminate casual divergences, and the average for the different observers indicates systematic divergences which spread over a range of 10·5 phons. But it is doubtful whether these systematic divergences can be regarded as permanently inherent in the observers. They may only hold for the day of the test. In the last column readings obtained 7 months previously, in which the observers were allowed to take their own readings, are given, from which it appears that the systematic divergences do not hold over that period. The mean of all the readings, 62·0, is in good agreement with the E.L. value of 62·5, as are also the average values for any of the four tests. The test carried out seven months earlier is in error by only 1·3 phons.

Table 7

Noise G. E.L. = 62·5 phons. Meter readings (corrected).

Observer	Test No.	1	2	3	4	Averages for observers	Previous test
C. R. M.		66	63	73	67	67	66
R. G.		58	59	55	58	57·5	64
D. H. T.		61	58	52	55	56·5	56
A. S. E.		62	64	61	61	62	68
A. J. K.		67	66	67	67	67	65
Averages for tests ..		62·8	62·0	61·6	61·6	62·0	63·8

Table 8

One-telephone Subjective Noise Meter

Test	Noise	B.S. phons	Group	Reading (mean)	Error	Standard deviation	Probable error
1	E	91	I	97·5	+ 6·5	5·5	1·2
2	F	79	I*a*	81	+ 2	6·2	2·1
3	G	62.5	I	76·5	+ 14	6·1	1·4
4	H	53	I	63	+ 10	10·9	2·4
5	I	26	I	33	+ 7	7·2	1·6
6	J	79·5	I	82	+ 2·5	5·8	1·3
7	L	75	I	80·5	+ 5·5	8·5	1·9
8	M	53	I	65	+ 12	7·3	1·6
9	N	95	I	103	+ 8	4·2	0·9
10	O	46	I	54	+ 8	10	2·2
11	P	88	I	93·5	+ 5·5	5·7	1·2
12	Q	103	I	107	+ 4	3·9	0·9
13	S	73	I	75	+ 2	6·4	1·4
14	T	80	I*a*	80	0	8·9	3·0
15	Transformer	70	4 obs.	75	+ 5	4·1	—
16	Transformer	61	4 obs.	65	+ 4	4·0	—

Note: The E.L. values of Tests 15 and 16 were obtained by the two-telephone method.

The results indicate that, for an important test, to have less than five observers is inadvisable and that to repeat the test once or twice and take the mean of all readings is useful in minimizing the effect of casual divergences. It is doubtful whether it is essential to prevent observers from taking their own readings, but it is a safeguard which should tend to greater confidence in the final value.

(c) One-telephone subjective meter.

The performance of the one-telephone meter having a pure reference tone of 800 cycles per sec. has also been examined. All readings were taken with the observer facing the source. The meter was adjusted to indicate correctly the E.L. of 1 000-cycle pure tones of high intensity. A number of readings were then taken for 1 000-cycle tones of various levels with group I. With the same group in all but two tests, where group I*a* was employed, readings were then obtained with the meter for various noises of known E.L. These are shown in Table 8 and plotted in Fig. 6.

If it be assumed that the law of the instrument when adjusted for high levels is linear, it is seen that the indications are too high. Although the noises are not evenly distributed over the E.L. range, the unsuitability of this assumption may be gauged by taking the average amount by which the indications are in error, which is 6·2 db., excluding tests 15 and 16. The maximum error is 14 db. If we assume the 1 000-cycle pure-tone curve to be a generally applicable calibration curve, the meter gives indications too high for levels above 80 phons and, in general, too low below 80 phons. The average departure from this curve, irrespective of sign, is 3·3 db., and, allowing for sign, − 0·4 db., which is considerably smaller than with the first assumption. The maximum departure is ± 6 db. Alternatively, a simple device is to apply a correction of − 6 db. to all readings. On this basis the average amount by which the indications are in error, irrespective of sign, is 3·1 db., and, allowing for sign, practically zero. The maximum error is + 8 db. Fig. 6 shows that a line displaced 6 db. from the dotted line fits the points on the whole, including the two transformer-noise points, rather better than the 1 000-cycle pure-tone curve.

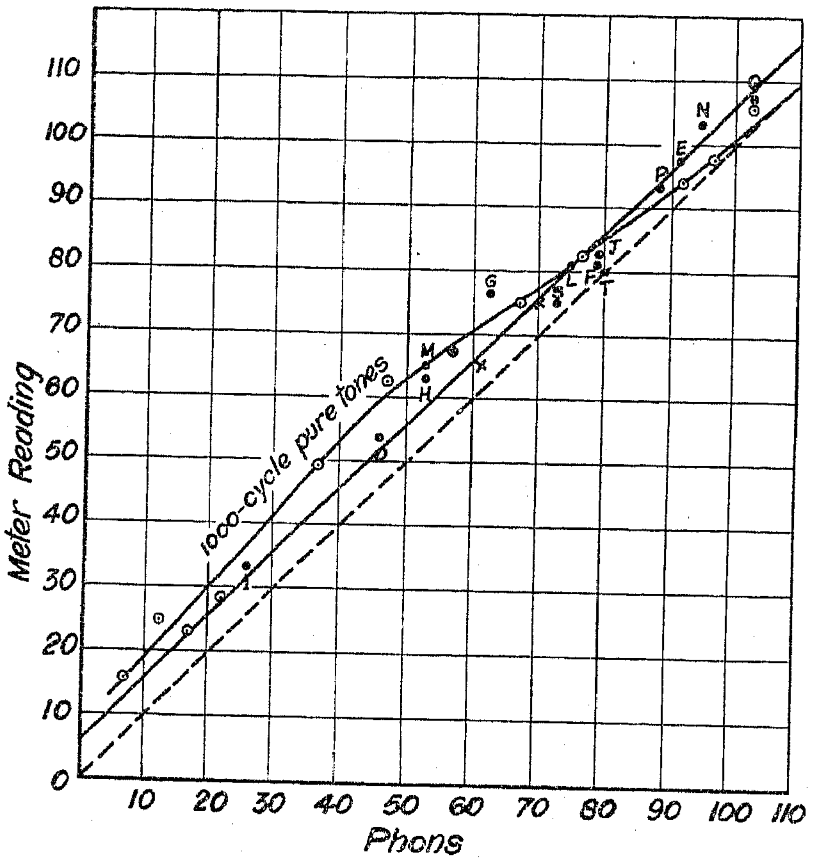

Fig. 6.—Results obtained with one-telephone subjective noise meter.

⊙ ⊙ 1 000-cycle pure tones.
● ◉ Synthetic noises.
× × Transformer noises.

The peculiar shape of the 1 000-cycle pure-tone curve must be ascribed to subjective factors associated with the mode of listening involved, i.e. listening simultaneously to one sound with one ear and another sound with the other ear, since the telephone receivers are known to have a linear response up to 110 db. The divergence of opposite sign at high and low levels between the pure tone and the noise readings suggests that the values obtained with this mode of listening may not be independent of the type of noise under observation. On the other hand the divergence may be fortuitous. Inquiry into these matters is more appropriate to the study of audition than to a search for noise-measuring methods suitable for engineering purposes.

(d) Comparison of one-telephone and two-telephone meters.

Table 9 summarizes the performances of the one-telephone and two-telephone subjective meters, the figures being based on 14 tests with the former and 16 with the latter.

Comparing the operation of the two meters from the point of view of an observer, the mode of listening with the one-telephone meter gives an impression that greater certainty of balance has been attained than that associated with the two-telephone meter. This is particularly the case at high levels. Yet this impression is not in accord with the experimental evidence. To illustrate the point we may take the average error, irrespective of sign, and the average probable error of observation for measurements with the two meters above and below a level of 75 phons. In estimating the former we will use the pure-tone calibration curves of the meters so that constant errors may be eliminated as far as possible. With the two-telephone meter and noises above 75 phons, the average error is 1·2 db., the corresponding average probable error being 1·1. Below 75 phons the figures are 2·4 and 1·6, respectively. For the one-telephone meter above 75 phons they are 3·3 and 1·6, and below 75 phons 3·3 and 1·8. Thus, based on both the ascertained average error and the estimated probable error of observation, the two-telephone meter is actually more trustworthy in its operation than the one-telephone meter at both high and low levels. Three factors may contribute to this result. First, the two-telephone meter avoids the abnormal condition of different sounds in the two ears heard simultaneously. Secondly, the criterion of balance is not loudness equality but absence of change of loudness where the sounds are heard alternately, which is likely to be more discriminating. Thirdly, binaural listening gives greater sensitivity to loudness differences than monaural listening.

Apart from the dispersion of the indications, the law of the two-telephone meter for levels above 30 phons is more nearly linear than that of the one-telephone meter, on account of the mode of listening being the nearest practicable approximation to that embodied in the definition of E.L.

The procedure of the two-telephone method is less convenient than that of the one-telephone method. The two methods can be embodied in one instrument and either method used according to the circumstances of the test.

(e) Objective Meters.

It would be highly desirable if noise could be measured by purely instrumental means such that any personal observational error was quite negligible. Meters consisting of a microphone, amplifier, and indicating instrument, have been made for some years and have been adjusted by weighting networks so that, when presented with single pure tones, they indicate in accordance with the equal-loudness relations adopted. If the relations are valid, the E.L. of pure tones will be correctly indicated. But, in addition, such meters have

Table 9

Basis of calibration assumed	Decibels	
	One-telephone meter	Two-telephone meter
Average error, irrespective of sign, assuming linear law	6·2	2·3
Average error, allowing for sign, assuming linear law	+ 6·2	+ 0·7
Average error, irrespective of sign, assuming pure-tone law	3·3	1·7
Average error, allowing for sign, assuming pure-tone law	− 0·4	+ 0·6
Average error, irrespective of sign, assuming linear law corrected 6 db.	3·1	—
Average error, allowing for sign, assuming linear law corrected 6 db.	0	—
Average probable error of observation	1·7	1·3

been used to measure complex noises, and the implied assumptions as to the law of summation of the human ear for such noises call for verification. In some of the meters it is assumed that an energy summation of the weighted components is correct and therefore a thermal instrument is used. (This implies that the relative phase of the components is unimportant. The authors have found that this is so, except perhaps at levels over 100 phons.) In other meters, instruments of the copper-oxide rectifier type have been found to give similar results to the thermal type and are therefore used because of their geater robustness and overload capacity. In examining the performance of objective meters, the authors therefore made measurements, whenever possible, by an apparatus, correctly weighted in respect of frequency for the level in question, and using a rectifier output instrument calibrated, in conjunction with variable attenuators, directly in db. In examining the performance of the objective meter the analysis of the sounds was known in most cases, so that the equivalent 1 000-cycle energies of the components were known and summation readily effected.

The relation of these two values to the E.L. values is shown in Table 10.

In choosing these noises to demonstrate the properties of the objective meter, the guiding principle was that, from the point of view of the ear and its summation of tones, noises can be considered as ranging from pure tones, through cases where one tone is more or less predominant, to groups of tones in which the tones of any one group are of equal loudness. It is a comparatively simple matter to adjust the sensitivity of an objective meter by means of weighting circuits so that it indicates correctly for pure tones in accordance with accepted equal-loudness relations, but a study of Fig. 4 shows that the adjustment of the sensitivity should be appropriate to the level of the tones in question. It is therefore necessary to provide a switch on the apparatus to set the weighting in accordance with the indication of the instrument.

Turning to the other extreme, Table 10 shows the effect of a group of tones all of equal loudness. It is seen that at moderate intensities the objective-meter indications are more and more in error, negatively, the more components there are present of approximately equal loudness. This suggested tests R and S, as saw-tooth wave oscillations contain many harmonics of intensity gradually decreasing with frequency. The very large errors of 19 and 23 indicate the extent to which the objective meter can be in error when measuring such a noise. The fact that the error is very much less in the case of noise T is due to the incorrect weighting of the apparatus at the very low frequency of 10 cycles per sec.

In practice, engineering noises may lie anywhere between these two limits of pure tones and "multi equal loudness" tones. Noise P is a typical turbo-generator noise of 10 important components, the remainder being omitted as they were small. In this case there is very good agreement between the calculated

Table 10

Noise	B.S. phons	Objective meter readings		Errors	
		Calculated	Observed	Calculated	Observed
A	61	53	56	− 8	− 5
B	37·5	33		− 4·5	
C	59	53	55	− 6	− 4
D	80	73		− 7	
E	91	88		− 3	
F	79	69		− 10	
G	62·5	48	50·5	− 14·5	− 12
H	53	38		− 15	
I	26	18		− 8	
J	79·5	68		− 11·5	
K	52	38		− 14	
L	75	58		− 17	
M	53	38		− 15	
N	95	95		0	
O	46	31		− 15	
P	88	75·4	76	− 12·6	− 12
Q	100	98·5		− 1·5	
R	91		72		− 19
S	73		50		− 23
T	80		74		− 6

Table 11

PERFORMANCE OF OBJECTIVE METER ON TRANSFORMER NOISE

(Sources A and B are 75 000-kVA transformers, type OFW, 3-phase, 11 000/66 000 volts. Source C is a 12 500-kVA transformer, type OFW, 3-phase, 66 000/6 600 volts.)

Test No.	Source	Analysis of noise (frequency/phons)	Summed equivalent energy	B.S. phons (2 tel.)	Error of meter
			db.		
1	A	100/55, 200/64, 300/47, 400/48, 500/38, 600/50, 700/40, 800/44, 1 100/39	65	70	− 5
2	A	100/38, 200/63, 300/44, 400/44, 500/40, 600/35	63	61	+ 2
3	B	100/70, 200/71, 300/81, 400/68, 500/64, 600/62, 700/59, 800/57, 1 000/48, 1 100/52, 1 200/44, 1 400/42	82	85	− 3
4	B	100/60, 200/69, 300/67, 400/64, 500/48, 600/52, 700/50, 800/48, 900/39, 1 100/43	72·5	77·5	− 5
5	B	100/65, 200/67, 300/67, 400/56, 500/44, 700/50	71·5	72	− 0·5
6	C	100/58, 200/64, 300/61, 400/41, 500/44, 600/54, 700/40, 800/38	66·5	68·5	− 2
7	C	100/64, 200/68, 300/72, 400/45, 500/58, 600/62, 700/50, 800/46, 900/45, 1 000/44	74·5	77	− 2·5

summed-energy value of 75·4 and the objective-meter reading of 76, but both are far from the E.L. value of 88 phons. When one tone is 5 or more phons above the other components of a noise the error of the objective meter is not so great. This is illustrated by some tests on two 75 000-kVA and one 12 500-kVA transformer given in Table 11.

In all except one case the instrument reads low by amounts up to 5 db. These errors are comparatively small, but it must be remembered that with other transformers or other acoustical conditions the effect illustrated in noises E to I of Table 4 may cause errors of up to 15 db.

Comparing the calculated and measured objective values of Table 10, two points of difference must be borne in mind. First, as has already been mentioned, the calculation is performed on an equivalent-energy basis and therefore amounts to taking the r.m.s. value of the equivalent 1 000-cycle pressures, whereas the rectifier meter yields something between the r.m.s. and average values of these pressures. Secondly, in the calculation the correct contour is used in obtaining the E.L. of each component, whatever its value, whereas the objective meter uses one contour, set in accordance with the meter reading for the whole noise, for all the components, irrespective of level. It is not anticipated that either of these points will cause large errors in practice.

The performance of the objective noise meter is summarized in Fig. 7. The three curves illustrate the increasing inapplicability of the law of the instrument as the number of component tones of approximately equal loudness in the noise increases. The effect is most marked at medium intensities, the divergence from the pure-tone values decreasing at high and low intensities.

It is clear, therefore, that in order to estimate the error of the objective-noise meter in any particular case it would be necessary to have a detailed analysis of the noise, giving the frequency and the magnitude in phons of each component. An approximation to the phon value could then be made by interpolation in Fig. 7 and comparison with Table 10. In cases where this is practicable, the need for the objective meter is eliminated. On the other hand it is possible to calibrate an objective meter for a given type of noise. In that way the instrument can be extremely useful as a routine check on the noise of a number of supposedly identical machines in a factory, where the time required for sub-

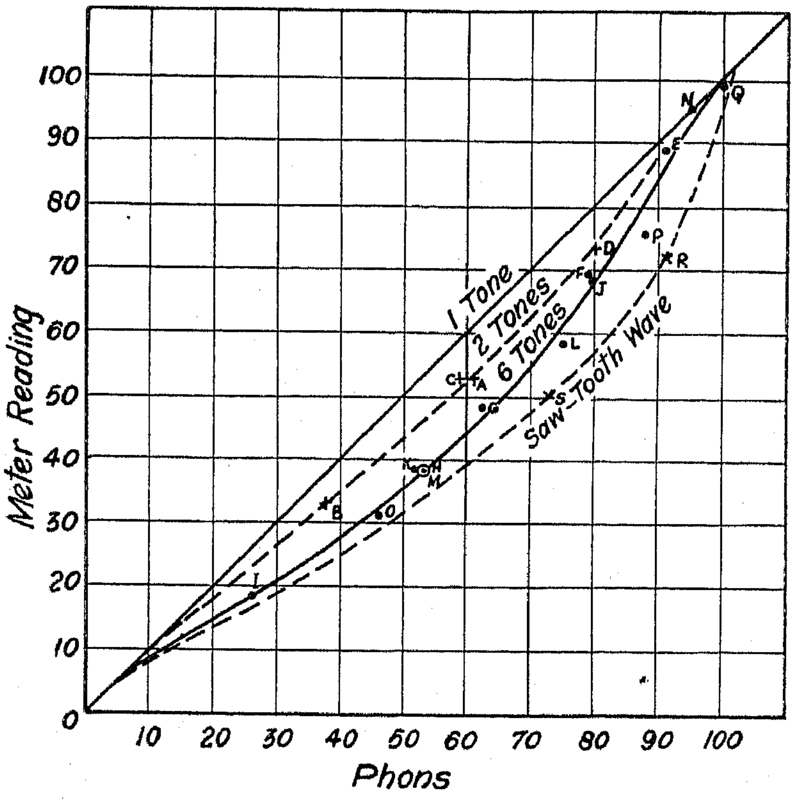

Fig. 7.—Results obtained with objective noise meter.

+ + Two equally loud tones.
● ● Six equally loud tones.
P Ten tones.
× × Saw-tooth wave.

jective tests would be prohibitive. The calibration could be effected in terms of the mean of the readings of 5 observers, using the 2-telephone apparatus, on machines covering the range met with in manufacture. Provided the character of the noises does not change greatly, such a procedure is quite justifiable.

Another case where the objective meter is essential is with a noise of short duration where there is not sufficient time for a subjective reading. If the duration is sufficient to give a steady reading on the meter, then the equivalent intensity of the noise can be given with the possibility of giving also the E.L. value, if the correction for the type of noise is known. If the duration of the noise is so short that the meter has not time to reach a steady value or if the noise is very intermittent, it is necessary to modify the time response of the meter so that it indicates correctly the E.L. of noises of that character. The authors have made no attempt to do this in the present paper.

(7) THE EQUIVALENT LOUDNESS OF NOISES ASSOCIATED WITH IRREGULAR SOUND FIELDS

In many practical cases met with in engineering noise problems the sound field associated with the noise to be measured does not consist of a plane wave or waves coming directly from in front of the observer, such as is envisaged in the definition of the phon. The form of the field may differ from this in varying degrees up to a completely diffused distribution. Irregular fields may arise from the manner in which the sound is radiated from a machine of complicated form or from a machine consisting of more than one unit, such as a motor-generator. Components of different frequencies may be radiated in different directions from different parts of the machine. If the machine is installed in a confined space or near a wall or other sound-reflecting objects, the field distribution will be further complicated by reflected waves. In general, it is only when measurements are made under conditions approximating to infinite space (e.g. in the open, in a lined test-room, or in a building very large compared with the dimensions of the machine) and at a distance from the machine several times its largest dimension, that an approximately plane-wave distribution is obtained. If observations are made on a noise of irregular field distribution by means of an objective noise meter, the indications will be arbitrarily dependent on the directional properties of the microphone used, which may be pronounced. The two-telephone subjective meter enables this difficulty to be eliminated. Its use in an irregular field enables the noise associated with the field to be expressed in terms of the standard 1 000-cycle plane wave which for a "normal observer" produces an equal loudness sensation. Thus the effect of the irregular field on the loudness sensation experienced by a normal observer is regarded as a constituent of the E.L. of the noise for the particular location and orientation taken up by the observer in the field.

Used for this purpose the two-telephone meter may be regarded as a link or transfer instrument between the ideal plane-wave distribution and the more or less irregular distributions which occur in practice.

(8) CONCLUSIONS

The value of a common standard to which the performance of all noise meters may be referred is evident. The B.S. phon enables a consistent, unifying system of noise measurement to be set up, relevant to the manner in which sounds are normally heard. It provides a definite objective for the development of improved noise meters, viz. that their indications shall be in terms of the standard. It also throws considerable light on the mode of operation of existing meters, which, in the absence of a standard, could only be conjectured. Knowledge of the defects of existing meters clearly contributes to the development of improved meters.

Experience indicates that under suitable laboratory conditions and using the technique described, representative E.L. values in accordance with the definition may, in general, be determined to within ± 2 phons with a group of 10 observers. The uncertainty may be further reduced by employing a larger group. Hence E.L. values of a precision adequate for present purposes are obtainable, since the errors of present noise meters are of a higher order. However, it is of interest to consider how the precision of the standard might be increased. If it were sought by way of an improved technique, a fundamental requirement would be the retention of the principle of the observers performing their essential task of judging relative loudness and no more, under conditions as simple and natural as possible. The object of the improved technique would be to enable an observer to give more consistent judgments and the several observers more unanimous judgments under these conditions. It is probable that the dispersion observed with the C.S. procedure arises from limitations inherent in the observers. With a given group, the effect of the inconsistency of individual observers' judgments can be minimized by taking the mean of repeated tests. The effect of systematic divergences between observers can only be reduced by employing more observers. This also tends to minimize the effect of individual inconsistencies. Hence an increase in the number of observers is likely to be the best means of increasing precision, and without considerably more than 10 observers an improved technique can hardly be of material value. With 20 observers and a standard deviation of 3 the probable error is approximately 0·5. At some levels this is already smaller than the minimum detectable change of intensity for the average person. At 800 cycles per sec., which is sufficiently near to 1 000, this quantity for binaural listening is approximately 1·9 db. at a level of 10 phons, 0·9 at 50, and 0·3 at 90. Hence it appears that ample precision is already obtainable if a sufficiently large group is employed.

With regard to equal-loudness relations, it is the authors' view that, in the study of noise as ordinarily perceived, the relations used should be determined directly for free-field conditions.

The indications of the conventional objective meter can be so wide of the mark that the authors have, as an intermediate step, explored the possibility of removing some of the practical objections to subjective secondary methods. The two-telephone meter enables a steady noise of any type to be measured in the field within limits which are sufficiently small for most technical purposes. Uncertainty may be further reduced, without employing an impracticably large group of observers, by the adoption of special precautions. The two-telephone meter forms a convenient means of measuring noise associated with irregular sound fields, and is not liable to arbitrary effects which may arise with objective meters when field irregularity is present.

The one-telephone meter is convenient in use but its indications are not so trustworthy and are liable to a correction the basis of which does not appear to be very certain.

Two general points may be noted. The authors' experience confirms the view that in subjective measurements the impressions of an observer do not form a trustworthy indication of the conclusiveness of his judgments and are often completely at variance with the recorded observations.

The subjective effects of telephone receivers embodied in noise meters should not be assumed but should be verified in terms of free-field conditions for the range over which they are used.

Considered as a generally applicable noise meter, the performance of the conventional objective meter is altogether inadequate. It does not compare noises of different E.L. and the same composition unless they consist substantially of pure tones. It does not compare noises of different compositions and the same E.L. Hence it must be used with caution and only when calibrated for the composition and approximate level in question. At 90 phons and over, the meter is fairly consistent and forms a valuable means of measuring loud noises. But since the object of research in engineering-noise problems is to reduce noise to moderate or low levels, the meter is of restricted value where it is most needed.

In conclusion, it is seen that the primary E.L. measurements have, as a whole, a substantially higher precision than is usual with field measurements by secondary subjective methods. The authors ascribe this to (*a*) the noises being accurately constant and reproducible; also (*b*) to the observers being free from the distracting influences of extraneous noise and fatigue, and (*c*) from the use of telephones and the making of adjustments. Further, (*d*), the normal mode of listening is used, the judgment being assisted by hearing the sounds under comparison in quick succession. In the laboratory measurements with the two-telephone meter, the advantages of (*a*), (*b*), and (*d*), remained, but the admission of (*c*) was accompanied by a loss of precision. The abnormal simultaneous listening to different sounds in the two ears involved in the one-telephone method was accompanied by a further loss of precision. Hence the authors consider that the choice of the B.S. phon as a primary standard of reference is justified not only by its relevance to the manner in which noise is normally heard, and hence to practical issues, but also by the higher precision of which it is capable, compared with the other subjective methods examined.

No system of measurement which is plainly incompatible with simple aural assessment will commend itself for engineering purposes, since in that sphere the aural criterion will prevail. Any obvious contradiction between measurement and considered aural impressions would lead to the rejection of the former. An arbitrary objective standard has sometimes been advocated. In the present state of the art, such a standard would introduce an aurally incoherent element, adverse to progress in both the study of acoustical science and its application to industrial problems. In the authors' view the B.S. phon constitutes a unifying and practicable subjective primary standard, affording a firm foundation upon which the future study of noise, both scientific and industrial, may proceed.

The subject of noise measurement is in a state of rapid development. Hence, now that a reasonably adequate primary standard is available to guide the development of noise meters in regard to performance, the authors are in favour of avoiding restrictions in the principles or details of construction of such meters, provided their performance conforms to the standard. In this way the evolution of meters technically adequate and practically convenient will be assisted.

ACKNOWLEDGMENTS

The authors wish to acknowledge the assistance and valuable suggestions they have received from Dr. R. Guelke, who has been responsible for the subjective measurements, and from Mr. C. R. Maguire, who has been responsible for the physical measurements. They also wish to record their appreciation of the forbearance of the members of Group I. They wish to thank Mr. J. H. Rider, consulting engineer, for permission to quote the test data on transformers, and Dr. A. P. M. Fleming, C.B.E., Director of Research, Metropolitan-Vickers Electrical Co., for permission to publish the paper.

APPENDIX I

Physical Basis of Measurements

The measurement of the frequency, intensity, purity, and field distribution, of the experimental tones is effected by condenser microphone and associated measuring and analysing circuits.

The condenser microphones are of a uniform pattern, $2\frac{1}{2}$ in. outside diameter and constructed with the diaphragm surface flush with the case. The diaphragm is $1\frac{7}{8}$ in. dia. The microphone, connecting leads, and other circuits, are completely screened. The microphone output voltage is fed into a battery-operated amplifier of adjustable gain. The amplifier output is fed into an electrical frequency analyser of the double turned circuit type described in a previous paper.* The analyser has a range of 25 to 25 000 cycles per second, is continuously adjustable over five overlapping ranges, and is highly selective. Two tones of equal intensity and differing by only 4 per cent in frequency may be measured to 1 db. The current in the second resonant circuit of the analyser is passed through a meter having a range of 20 db. This covers the 10-db. steps in which the gain of the amplifier is adjustable. Suitable meters and controls are provided so that the anode currents and filament voltages of the various valves may be adjusted to fixed values. This, combined with the conservative design of the amplifiers, gives day-to-day constancy of amplification to 1 db. However, for basic measurements, this constancy is not assumed. A potentiometer, fed from an adjustable-frequency oscillator, is provided so that voltages of known frequency and magnitude may be applied to the first stage of the amplifier. By this means an overall check of the combined amplifier and analyser calibration can be quickly obtained at any frequency and level. This check is carried out at every basic equivalent-loudness determination.

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* B. G. Churcher and A. J. King: "The Analysis and Measurement of the Noise Emitted by Machinery," *Journal I.E.E.*, 1930, vol. 68, p. 97.

Frequency determinations are ultimately based on a valve-maintained tuning fork of 1 000 cycles per second, the frequency of which is known to a few parts in 100 000. Multiple and sub-multiple frequencies are compared with the fork by means of a cathode-ray oscillograph.

The microphones are calibrated by means of the Rayleigh disc. Pressure calibration is carried out by the resonant-tube method. Two tubes are used; a large tube having a range 25 to 1 000 cycles and a small tube of the minimum practicable diameter, i.e. the diameter of the microphone diaphragm. This tube has a range of 250 to 9 000 cycles. The discs consist of a microscope cover slip in the large and a mica disc in the small tube. Advantage has been taken of a technique developed by the Metropolitan-Vickers Electrical Company by means of which a small aluminium film is deposited directly on the disc, forming a high-grade mirror. By this means a sharply-defined light spot is obtained and some difficulties which arise with the usual methods are avoided. The glass suspensions used give a very stable performance. The restoring torque of the suspension is obtained from a torsion pendulum measurement. The authors have availed themselves of recent work on the errors of the Rayleigh disc, and the proportions of the discs used have been chosen to make these errors as small as is practicable. Corrections have then been applied for the residual errors. From a consideration of the various errors involved in the calibration, the overall error of the final calibration constants is not thought to exceed 2 per cent. Although the discs and suspensions of the two tubes are of quite different dimensions, a comparison of pressure calibrations obtained by the two tubes shows agreement to within 2 per cent over the range 250 to 1 000 cycles. An unpolarized condenser microphone has been found to be very satisfactory as a double-frequency sound source. The purity of the source is an important matter and particular attention was paid to the design of the oscillator used to supply the source. The purity of the pressure wave set up in the tube is examined at every calibration. At a given frequency, readings are taken at several pressures within the range of the disc to detect any possible non-linearity. The field calibration is carried out in a lined cabinet. A substitution method is used and arrangements are provided by means of which substitution of disc for microphone or vice versa may be effected without opening the cabinet. In this way disturbance of the thermal conditions is avoided. The field distribution in the operating region was explored by means of a fine probe tube and microphone and the absorption and other conditions adjusted until adequate freedom from irregularities was obtained. Field calibration is carried out over the range 500 to 9 000 cycles. It is found that the field calibration is consistent with the pressure calibration at a given frequency, taking into account the increased sensitivity to be expected as the wavelength approaches the microphone diameter.

APPENDIX II

Experimental Sound Sources

Given a test room of adequate absorption, the main problem in setting-up sounds for basic E.L. determinations is to secure adequate purity at high intensities. As stated on page 5 three sources, A, B, and C, have been used. The choice for a given measurement depends on the characteristics of the sound in relation to the properties of the loud-speaker. A schematic diagram of the circuits employed is shown in Fig. 8. If the special changeover switch described in Section 3(a) is thrown over to one position the voltage of a 1 000-cycle oscillator is applied, through attenuator B, to the first stage of a power amplifier, the output of which is applied to the loud-speaker through attenuator A. The reference tone is thus set up. If the switch is thrown over to the opposite position the voltage of an oscillator or other appropriate electrical source is applied to the amplifier through attenuator C, thus setting up the test sound. The intermediate switch position disconnects both sources. D is a push-pull oscillator of adequate purity. B is adjustable in 1-db. steps and is used to vary the intensity of the reference-tone presentations during an E.L. measurement. The power amplifier is of 24 watts output and is adequate for producing the loudest tones required. Attenuator A is a convenient means of attenuating both sounds by the same amount. This, in conjunction with C, enables the test sound-level to be adjusted.

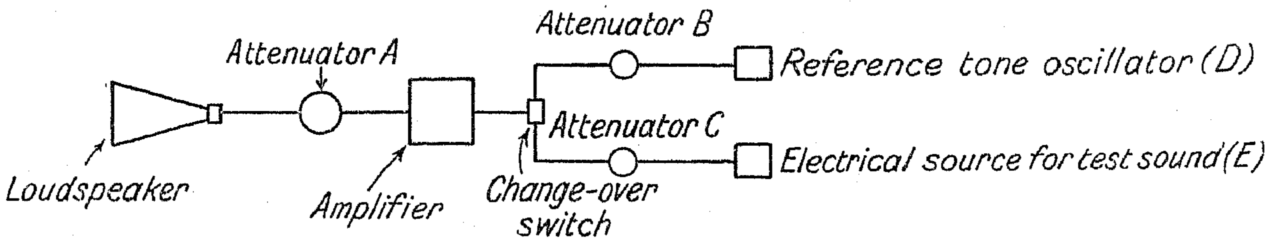

Fig. 8

The nature of E depends on the type of sound desired. For pure tones a push-pull oscillator continuously adjustable, in five overlapping ranges, from 40 to 25 000 cycles per sec. is used. For complex non-harmonic sounds 6 push-pull oscillators of nominally fixed frequency have been constructed. Each is provided with a coarse and fine attenuator, so that the relative intensity levels of the components of the sound may be independently adjusted. The frequencies of these oscillators can, if required, be re-set for other values by adjusting the capacitance and/or inductance in circuit.

For the production of harmonic complex sounds an electromagnetic tone generator has been constructed. Considerations of space permit of it being described in outline only. It consists in effect of 10 small alternators mounted on a common shaft. Their frequencies are 120, 240, . . . 1 200 cycles. Cobalt-steel magnets supply the excitation. Across the armature winding of each alternator is connected a resonant circuit to purify the wave-form of the terminal voltage, and a potentiometer resistance, by means of which any desired fraction of the terminal voltage may be tapped off. Means for adjusting the relative phases of the voltage are also provided. The voltages of the 10 alternators are connected in series and applied to attenuator C.

The set is driven by a shunt-wound d.c. motor. The d.c. supply is provided by a shunt-wound d.c. generator driven by a 3-phase induction motor operated from a power system of large capacity. The speed of the driving motor depends only on the supply frequency, which is very constant, and is independent of a.c. supply voltage within fairly wide limits. Hence the d.c. generator voltage is very steady. However, even with steady voltage, gradual changes in winding resistances due to heating and changes in the motor load due to brush and bearing friction variations would cause changes in the speed of the tone generator. A 1 000-cycle single-phase synchronous motor, of similar construction to the alternators, is therefore mounted on the same shaft. This motor is supplied from a 1 000-cycle valve-maintained tuning-fork operating in conjunction with a power amplifier. As the synchronous motor is capable of exerting sufficient torque to counteract any of the ordinary causes of speed variation, the tone generator is held in synchronism with the fork, the frequency of which is practically invariable. Synchronism is indicated by a stroboscopic device.

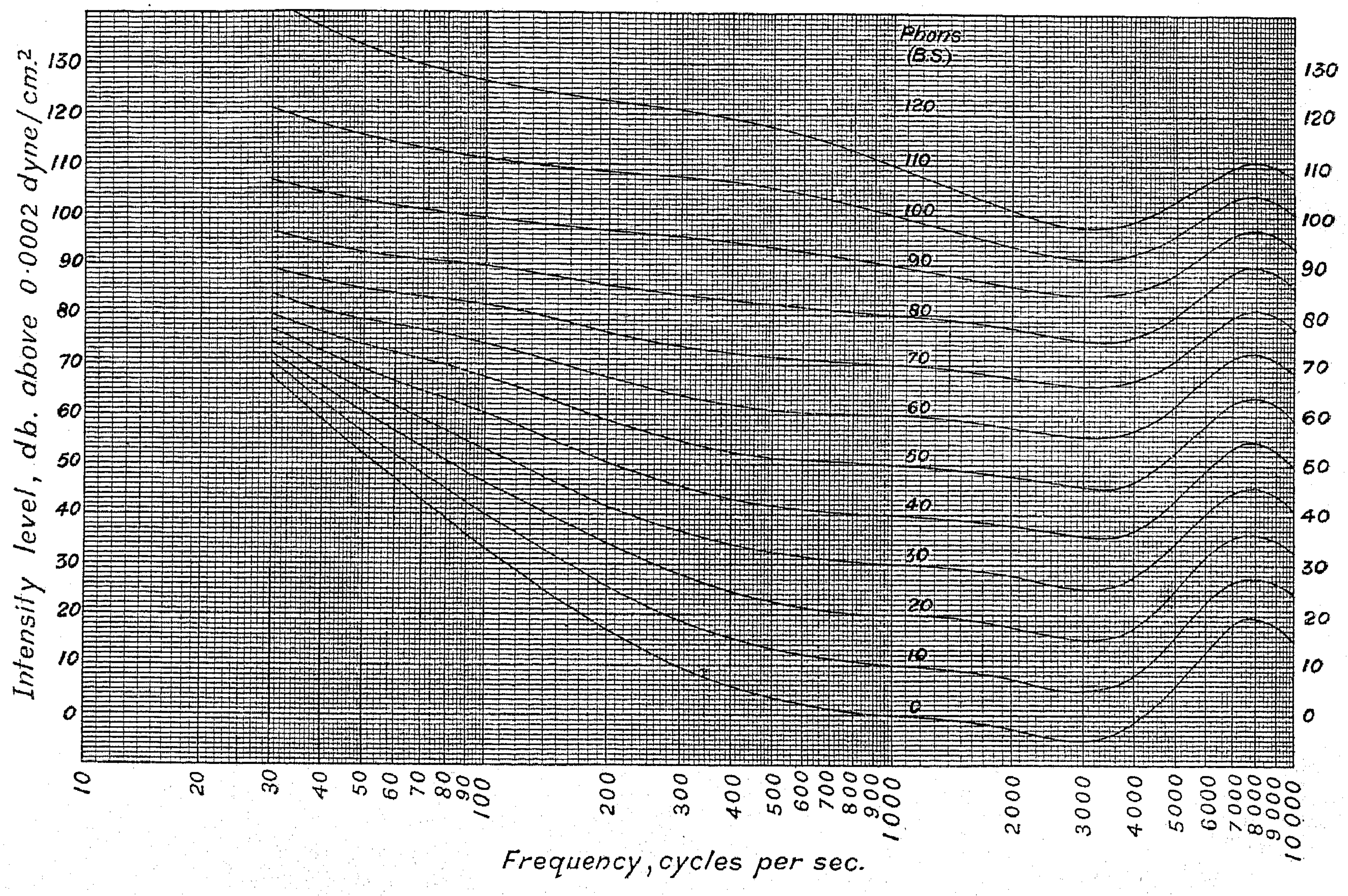

Fig. 9.—Equal-loudness relations, determined under free-field conditions.

Thus, by means of this apparatus, harmonic noises containing up to 10 components of any relative intensity may be set up. The practically invariable speed and the permanent-magnet excitation, together with a very stable amplifier, enable harmonic noises of a high degree of constancy to be set up. This is an important aid to precision in basic measurements.

APPENDIX III

Extended Free-Field Equal-Loudness Relations

(*Received* 21*st April*, 1937.)

It is well to recall that the drawing of equal-loudness contours involves the supposition that if, with three tones of different frequencies, tones A and B are equally loud and also tones A and C, then tones B and C are equally loud. If this were not so, doubt would be cast on the significance of the conception of two sounds being equally loud. Out of curiosity, the point was examined with a group of 10 observers in the early stages of the investigation. A level of about 85 phons was employed. Two tones, of 2 500 and 4 500 cycles respectively, were each measured in terms of a 1 000-cycle tone. The 2 500- and 4 500-cycle tones were then directly compared. The overall probable subjective error of the comparison was estimated to be 1 db. The observed discrepancy in the mean values was 2·6 db., which could be accounted for by the dispersion, but it was more likely to be due, in part, at least, to the enhanced physical errors which arise in high-frequency tone comparisons and which may not be included in the estimated probable error. This is suggested by a further comparison carried out with tones of 1 000, 500, and 150 cycles, where the physical conditions are far less critical. Here the estimated probable subjective error was also 1 db. but the discrepancy in the mean values was 0·2 db. Hence the tests confirm that two sounds which are each as loud as a third sound are as loud as one another.

Since the original free-field equal-loudness relations were determined, improvements in experimental conditions have enabled the range of the data to be extended. A set of relations derived entirely from free-field measurements has now been plotted, as shown in Fig. 9.

Improvements in the sound source enabled determinations at over 100 phons to be made. By extrapolations of a few db. the 110-phon contour has been deduced down to 120 cycles. For lower frequencies a somewhat larger extrapolation has been needed. At high frequencies field conditions have been improved, and the use of a head-rest has minimized the effect of variation in the position of an observer's head in the field. In addition to the high-intensity determinations, further values at 4 500 and 9 000 cycles have been obtained which are concordant with the original values. A set of values for 2 500 cycles has also been obtained. To verify the downward bend of the contours above 8 000 cycles, which is suggested by the 9 000-cycle data, some measurements were made at 12 600 cycles. The tone was clearly audible to all the members of Group I. Large dispersions were obtained, as expected. At one level tests were made with source B and with a small source D. At this frequency the latter gave a much more uniform field than the former and, taking this and the large dispersion into consideration, the agreement in the mean intensity-level values was as good as could be expected. The 12 600-cycle measurements confirmed the downward bend of the contours. The ages of the observers varied between 21 and 37, the average being 31 years. To some observers tones of 18 000 or 20 000 cycles are audible. Since inaudibility is ultimately reached as frequency is increased, it seems not improbable that the intensity level for a given E.L. ultimately increases indefinitely, i.e. the contours bend up again before 18 000 or 20 000 cycles is reached. (The upward bend of the contours at high levels as the lower-frequency region of inaudibility is approached may be noted.) It was found that a few persons of 45 years of age and upwards who were asked to listen to the 12 600-cycle tone were unable to hear it. A peculiar effect was encountered in attempting to obtain 8 000-cycle values, where a minimal aural sensitivity appears to occur. It was found that, at the normal inclination of the head, the loudness sensation was critically dependent on inclination—far more so than at 6 400 or 9 000 cycles. The dependence was so critical that useful results could not be obtained at 8 000 cycles. It may be noted that Sivian and White* have found a maximum difference in threshold pressures for a spherical field and one of random incidence at 8 000 cycles, thus showing a minimum collecting power of the ear for normal head inclination at this frequency. This minimal sensitivity appears to be a direct consequence of the proximity of the critical to the normal head inclination.

Equal-loudness determinations at the highest frequencies being considerably dependent on the age of the observers, there is little to be gained by further refinement of technique in this region for equal-loudness relations intended for general use. For frequencies below 1 000 cycles a consideration of all the possible sources of error suggests that the relations are applicable to the " normal observer " (and therefore to people in general) to within ± 2 db. The authors have found that the use of the data of Fig. 9 has removed many anomalies, particularly those encountered in the correlation of the E.L. of the components of machinery noise, obtained by sound analysis, with the E.L. of the whole noise.

DISCUSSION BEFORE THE METER AND INSTRUMENT SECTION, 12TH FEBRUARY, 1937

Mr. W. West: Meters for measuring noise have been in use for many years, but the subject of noise measurement remains in a highly controversial state. The reason appears to be that the quantity we are measuring, loudness, is not a physical quantity but a sensation; that is to say, the question is one of opinion rather than of fact. If the measurements themselves are based on opinion, it is obvious that the whole subject lends itself to interminable discussion of an inconclusive nature. It appears to be necessary to discover a satisfactory, readily reproducible, and representative, average opinion.

In this country, as is described in Section (2) of the paper, a useful step has been made by the formulation of a standard—the British Standard Phon. Similar work has been done by standardization authorities in other countries, and this is to be followed up and, I hope, consolidated, by an international conference on acoustics. In my view, the usefulness of an international standard for noise measurement will depend more on its practicability and on the degree of unanimity of its acceptance than on the purely technical details involved.

By way of illustration, I will mention a few of the possible diverse opinions that may be held with regard to noise measurement. If the purpose of measurement is for noise abatement, it may be contended that the first step towards a scientific study of the problem must be the development of a proper technique for measuring the loudness of the noise. On the other hand, the view may be taken that loudness, being a sensation, is not analogous to any physical quantity and that the results of measurement of loudness will have little value since they cannot be used to decide what is a practical or economic limit to be set on any particular noise-reducing treatment. Some people may regard the loudness, by itself, as a relatively insignificant measure of the annoyance caused by noise, citing the case of a neighbouring loud-speaker the sound from which arrives at a relatively low loudness level but may possibly cause relatively great annoyance. Others could reply that annoyance is in any case a matter of psychology and circumstances, and, since loudness is the magnitude of the sensation, a measurement of the loudness provides all that can be attributed to the noise itself as a potential source of annoyance. In some cases another aspect of the noise problem may predominate, namely the masking effect, e.g. the preventing of good listening conditions in an auditorium or on the telephone. It cannot necessarily be assumed that the loudness of the noise is a more suitable gauge of this effect than would be any other arbitrary measure of magnitude. Or again, there is the point of view that the measurement should be transferred from the realm of subjective testing (opinion) into that of objective testing (fact) by the adoption of a standard, specifiable objective meter. The contrary view would maintain that although the objective meter provides facts rather than opinions, yet the relevance of these facts is as much a matter of opinion, and indeed they may be misleading.

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* L. J. Sivian and S. D. White: *Journal of the Acoustical Society of America*, 1933, vol. 4, p. 288.

These few examples illustrate the possibilities of wide divergences of opinion and the need for a willingness to compromise for the sake of standardization. They also indicate that different methods of measurement may be more or less suitable for different purposes of measurement.

The paper may give the impression that the problem of noise measurement is an easier one than it actually is in its generalized form. The majority of noises are not sustained for long periods at one level, but fluctuate in an irregular and often violent manner. Such are the noises of everyday life with which a telephone service has to contend.

The general extent of agreement between the results by different observers, as quoted in the paper, appears to be somewhat more close than is usual in our experience. For example, three practised observers, using a "one-telephone" subjective meter, carried out an extensive survey of noise at telephone locations in London. The survey occupied about 3 months and throughout this period repeated calibrations were made in the laboratory, the observers taking readings of a standardized noise at different known loudness levels. Inspection of the record of these numerous calibration tests showed that at a moderate level (where the divergence was widest) the average for one observer differed from that for another observer by 15 phons. The individual readings of any one observer were spread over a range also of about 15 phons. We have always adopted the principle of having separate calibrations for each observer using the meter, whenever possible, rather than associating a single calibration with the meter itself.

Mr. C. A. Mason: I am firmly convinced that subjective measurements of the nature outlined in the paper are fundamentally correct, but the authors make one or two statements which rather worry me. According to the tables either 10 or 5 observers were used in their tests; I find that it is necessary to use a large number of observers in order to obtain very consistent results, a method which is rather cumbersome in practice. The chief trouble with subjective readings is persuading people to believe the results when they have been obtained. There is a statement in the paper which rather confirms this. It therefore appears that there are considerable practical difficulties in the application of subjective methods.

The phon values which we obtain for a noise do not express the loudness of that noise. We who are accustomed to acoustic measurements know—when a noise level of 60 phons is quoted—how loud it will sound, but there is no simple relation between phon value and loudness. In Fig. 7, showing the average results obtained with the objective meter, the lower curve gives the average value indicated by an objective meter for six tones. As the phon value does not give us an accurate idea of the loudness of the noise, why not start off with the lower curve? Readings taken using this curve might be called "objective phons," to differentiate them from normal phons. The dotted lines in Fig. 7 approximately represent the deviations which we are likely to obtain from objective measurements. Some investigations which I have made show that in cases where the objective meter fails, such as transformer noises, the deviation obtained in subjective measurement is much greater than with the types of noise where the objective meter gives more accurate results. We once tested a transformer, a motor, and a complex tone from a loud-speaker; we had 20 observers, who were asked to place the noises in order of loudness. All the loudness values were within a band of 10 phons, but the judgments which we obtained were not at all consistent. This argument is submitted as objective readings are easily made and reproduced. Such readings do not give us a direct measure of loudness, but neither do subjective readings in phons, so that it is probable that such readings would be at least as useful as subjective measurements, and could be obtained much more easily.

I should like to support the statements made by the authors on the use of objective meters for routine testing. In our works we have calibrated objective meters, after taking subjective measurements of representative samples, and have successfully used the objective measurements for checking routine production and also as a rejection test. We have had these objective meters in use on gramophone motors for some time, and they have proved extremely useful in this connection.

There is a statement in the paper to the effect that if measurements are not taken under free-space conditions then an objective meter fails, because of the directional properties of the microphone. We can get over this difficulty nowadays by using non-directional microphones; we have recently built up a meter using this type of microphone, and we have found that the differences between the two methods are smaller than with the old condenser type of microphone.

Another point which is in favour of the subjective method is that when we are taking readings with an objective meter the background noise must be at least 10 db. below the noise from the machine; otherwise our readings will be very seriously affected. With the subjective measurement it is possible to get rid of the background noise, even if it is of the same order as the noise from the machine, because the ear possesses the discriminating property of being able to pick out the noise which it is required to measure. I have often measured noises in these circumstances, which are unavoidable when doing the work in a factory. I should like the opinion of the authors as to the amount of errors which is likely to be introduced in such cases.

Prof. R. A. Fisher: With respect to Fig. 3, the frequency with which the subjects say that the comparison noise is louder than the noise to be tested is an increasing sigmoid curve, which will generally be well represented by the integral of a normal curve of error. If we invert the half of this integral curve, for which the judgments "louder" exceeds 50 %, we arrive at a pointed-top curve which is not unlike a frequency curve and is treated as such in Fig. 3; it is not properly to be regarded as a frequency curve, although it is derived from the normal curve of frequency of errors. I think that the slight curvature shown at the top of Fig. 3 is really only due to chance irregularities of a series of measurements.

With respect to the precision of any measurement or system of measurements, there are, of course, two components. One is the precision with which the individual's own assessment has been arrived at by, let us say, 20 answers "Yes" or "No" scattered over this range, and the other is the extent to which the different subjects agree in their assessments. Even if all the subjects were exactly alike, and if the assessment of each was not exactly determined by experimentation—of course, with 20 or 30 answers one cannot get an exact estimate—still the values arrived at would be scattered in a normal curve of frequency of error. That the methods described in the paper do not afford an adequate basis for finding the precision with which any individual judgment has been ascertained is unimportant, because what we need to know—namely, the overall error due both to individual differences and to errors of measurement—is given by the scatter of the results obtained from the different individuals. I suggest, therefore, that one need not concern oneself with an elaboration of the scoring system, such as is shown in Table 2, but that one merely needs one estimate ascribable to each individual, which can be arrived at by a variety of quite simple methods. It would seem to be quite sufficient to obtain for each subject that phon value for which the judgment "Quieter" is given for higher intensities as often as the judgment "Louder" is given for lower intensities. This would be justifiable on the assumption that the frequency of application of the stimuli is constant over the central range in which the individual shows any doubt as to whether the value is louder or quieter.

On page 63 there is a formula due to Yule which at first sight appears to be nonsense, because if $n = 1$ the magnitude of the probable error is apparently infinite; nevertheless, some single observers whose results are recorded in the tables which follow have given uncommonly good estimates of the phon values. I assume that the explanation is that Yule used a convention in regard to the estimated value of S, that his S value was arrived at from the sum of the squares of the deviation from a mean divided by n. He knew very well that he ought to have divided it at that stage by $(n - 1)$, and so he made that correction in the derivation of the estimated error.

Dr. E. Billig: There is a good deal of confusion, even in engineering circles, about some fundamental terms used in acoustics, especially "intensity," "equivalent loudness," and "loudness." It is rather unfortunate that we have not discriminated more clearly between these last two terms, which have nothing to do with each other. The one gives the intensity of an equally loud standard tone, while the other refers to personal impression only.

The introduction of the logarithmic system (decibel and phon system) was based upon the validity of Fechner's general law, namely that psychological sensation is proportional to logarithm of stimulus. It has since been found that this law does not hold for acoustic sensations. For instance, a pure tone of 80 db. does not cause double the loudness impression given by the same pure tone of 40 db. intensity. Actually, it has been found by some investigators that an addition of about 9 db. gives double loudness. If this is so, then there is no reason to maintain the logarithmic scale, and a simple power law relating to sound pressure and loudness can be put forward.

The situation at present is that, whereas the "intensity" of a noise as defined in the standard specifications has a definite physical meaning inasmuch as it describes the quantity of sound pressure or sound energy, the term "equivalent loudness" has no direct physical or psychological meaning. This situation becomes obvious if we examine the difficulties the authors found when trying to comply with the present standard definition of equivalent loudness, in making measurements of noises encountered in practice; that is, of noises which are composed of a mixture of pure tones of different frequencies and intensities.

There are several possible reasons for these difficulties. Although the ear itself is characterized by physical dimensions and quantities, the process of hearing comprises, apart from such physiological processes as nerve conduction and reaction of the brain, some purely psychological factors which hitherto it has not been possible to simulate by an inanimate piece of apparatus. There is the well-known fact of subjective tones: if one sounds on a piano all the harmonics of a deep fundamental tone without actually playing the fundamental itself, many people, especially musicians, will feel quite certain that the fundamental tone has been played. I take it that these subjective tones play a big part when one is judging composite noises. These factors, being purely psychological, will depend largely on the individual observer. It should therefore be rather difficult to compare a complicated noise with a pure tone and decide which is the louder.

It is therefore no wonder that, until now, no instrument has been designed which would give accurate equivalent-loudness figures under all conditions. I attribute this not to a lack of suitable instruments but to the nature of the rather arbitrary quantity known as equivalent loudness.

For the use of the practical engineer, who has to deal mainly with noise reduction or with the measurement of transient traffic noises, there can be no doubt that a suitably weighted objective meter is the only feasible type, even if it gives comparative results only.

Mr. A. C. Hutchinson: This paper establishes two important points in regard to the present state of the art of noise measurement: firstly, that there is no objective method of measuring noise which will give absolute results; secondly, that even with the best subjective method available it is so difficult to obtain accuracy that subjective measurement by the ordinary commercial user of noise-measuring instruments is almost ruled out of consideration.

I would draw attention to the tests recorded in Table 7, in which 5 skilled observers were required, under ideal listening conditions and taking a total of 20 measurements, to get a result correct to about 0·5 phon or 4 per cent in loudness, the dispersion of individual readings in each group being so great that no confidence could be placed in the averages of a smaller number of readings. It appears from this evidence that, except in the laboratory, the *accurate* subjective measurement of noise is still impracticable.

In order to keep the question of noise measurement in perspective, I would invite consideration of Fig. A, which gives an example of a commercial as opposed to a laboratory noise measurement. The measurements here shown were undertaken to give some idea of how the noise of a turbine gear varies with its running speed. The results are, I think, typical of what an observer of *average intelligence, working by himself* and under commercial conditions can achieve with a subjective meter. There is an enormous spread in the readings, but it must be remembered that this spread is exaggerated because the readings are expressed not in phons but in the "loudness numbers" of Churcher, King, and Davies. In this connection, I wish to record my opinion that this scale of loudness is a most outstanding contribution towards the practical interpretation of noise measurements.

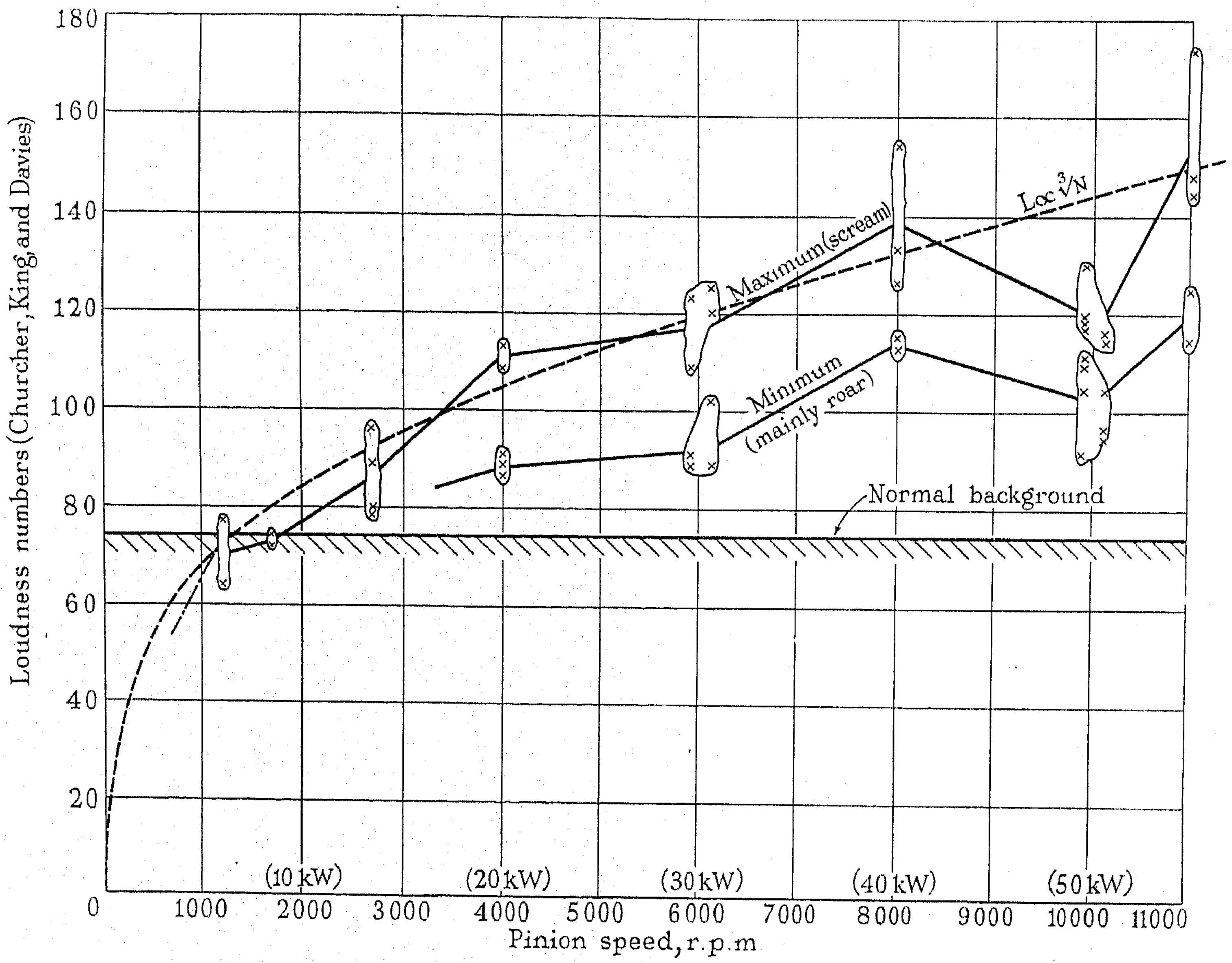

Fig. A.—Noise/speed relation of 50-kW turbine gear at full tooth load (475 lb.). Measurements taken at 6 ft. from wheel side of gear case by aural-balance technique, using 800-cycle reference tone. The dotted line shows curve which would result if loudness were proportional to cube root of running speed.

It is a little disappointing to those dealing with practical noise problems that the authors' work should be concerned so largely with the abstract side and should contain such a relatively small contribution towards the pressing need for the development of a reliable objective meter.

They make one or two tentative recommendations which give a hint as to the lines on which a rational objective noise-meter could be constructed. For instance, I gather that it is now considered that the loudnesses of the various components of complex tones are additive (loudnesses being approximately proportional either to the fifth powers of phon values or to the fourth roots of intensities), and on page 65 the authors make use of this. This conclusion leads to the following rather striking deduction: we know that if two sounds of exactly equal frequency are sounded together the resulting intensity level rises by about 3 phons or, say, 20 per cent in loudness. The authors contend that if those two sounds are sufficiently different in frequency the loudness doubles itself. It would be very interesting, I think, to know how much difference in frequency is necessary to change this loudness-increase from 20 per cent to 100 per cent.

It is stated by the authors that in regard to the performance of an objective meter the error or correction due to a number of tones is greatest for medium levels and least for high and low. I suggest that on the data presented in the paper there is no justification for saying that it is less at low levels. The noises O, B, and I, in Fig. 7 represent much the same percentage error in loudness as the other noises at medium levels.

Mr. B. G. Gates: In Table 1 it is mentioned that 5 men and 5 women were employed in Groups Nos. II and III, and I should like to know whether the authors have observed any systematic difference between men and women observers in that sort of measurement. My own impression is that women are more sensitive to high-frequency noises than men, i.e. that the right-hand portions of the curves in Fig. 4 will be lower for women than for men. It is common in my experience to find that the tone control on the wireless set is always turned to " mellow " by the lady of the house.

With regard to the statement on page 68 about the change of 0 to 90 phons at 120 cycles per sec. being 56 db. in the case of telephones and 70 in the case of direct measurements, I should like to offer the suggestion that the low difference observed in the case of telephones may be due to bone conduction through the head.

One of the observers dealt with in Table 7 appears to give quite consistent measurements, but during the period of 7 months he alters his impression by 7 db. I have often wondered whether subjective measurements are affected to any extent by the state of health of the observer. I have made a good many measurements of this sort myself, and I have to admit that my dispersion of readings is always worse after lunch. Again, in Fig. 7 it is noted that above 100 phons—though the authors did not go very far above that level—the errors dwindle away almost to nothing. At that level the ear response characteristic appears to flatten out, and I suggest there may be some connection between these two facts.

I should like to ask whether the authors have any information about the use of objective meters in assessing the benefits obtained by the soundproofing of buildings. The general effect of soundproofing is to change the spectrum of the noise and more usually to attenuate the higher frequencies, and if an objective meter is used to assess the benefit of the application of soundproofing there may be errors due to the change in the spectrum.

Finally, I should like to offer a suggestion which may possibly help to improve the accuracy of objective meters. I suggest that between the microphone and the amplifier a series of simple octave filters should be interposed—I find that about eight will cover the necessary spectrum—and readings could be taken simply by moving a knob to the eight positions. Before applying this plan to the method described on page 65 of combining noises to obtain a figure for the total noise, it is necessary to check the validity of the curve used.

(*Communicated*) Table 4 gives the equivalent loudness values for a large number of complex noises of specified composition. I have found it possible to work backwards from the results for the simpler noises and deduce a curve of loudness numbers such that the loudness number for a complex noise is the sum of the loudness numbers for the individual components. This curve is given in Fig. B, and it differs widely from the author's 5th-power law. In Table A I have applied this curve to the first 17 noises given in Table 4 of the paper; the results show remarkable agreement, in most cases to within 2 phons, and in no case does the divergence exceed 3 phons. These differences are of the same order as the errors of measurement of the component and total equivalent loudness.

Table A

Noise	B.S. Phons	Value calculated from Fig. A
A	61	59
B	37·5	39
C	59	59
D	80	78
E	91 (av.)	91
F	79	81
G	62·5	63
H	53	53
I	26	26
J	79·5	80
K	52	53
L	75	73
M	53	53
N	95	98
O	46	46
P	88	86
Q	103	101

This result, combined with my suggestion for the use of octave band-pass filters, definitely opens up the possibility of an accurate objective noise-meter employing a suitable number of filter pass-bands, say 8, and using Fig. B to combine the results and obtain the total equivalent loudness. Errors will occur where several components lie within the same octave, but they will be much less than those obtained with ordinary objective meters.

Mr. D. McMillan: In reading the paper I have been struck by the authors' comments on practical noise measurement in the engineering field. They obviously do not favour the adoption at the present time of an arbitrary standard objective meter, mainly because such a meter will not measure equivalent loudness. I must confess that I have not been convinced by their reasoning in this respect. In this connection there are points on which I should like the opinion of the authors.

One arises from their statement " The performance of the ideal noise meter would be independent of the type and level of noise over a wide range, in the same way that a satisfactory a.c. voltmeter indicates correctly over a wide range of frequency and waveform." Such a " satisfactory " a.c. voltmeter, if applied to a telephone circuit, will not give a reliable indication of the potential difference existing across that circuit due to the passage of speech currents. This has been such a difficulty in the past that telephone engineers have had to express this particular potential-difference in terms of the reading of an arbitrary specified voltmeter, although such readings cannot be referred back to the standard unit of potential.

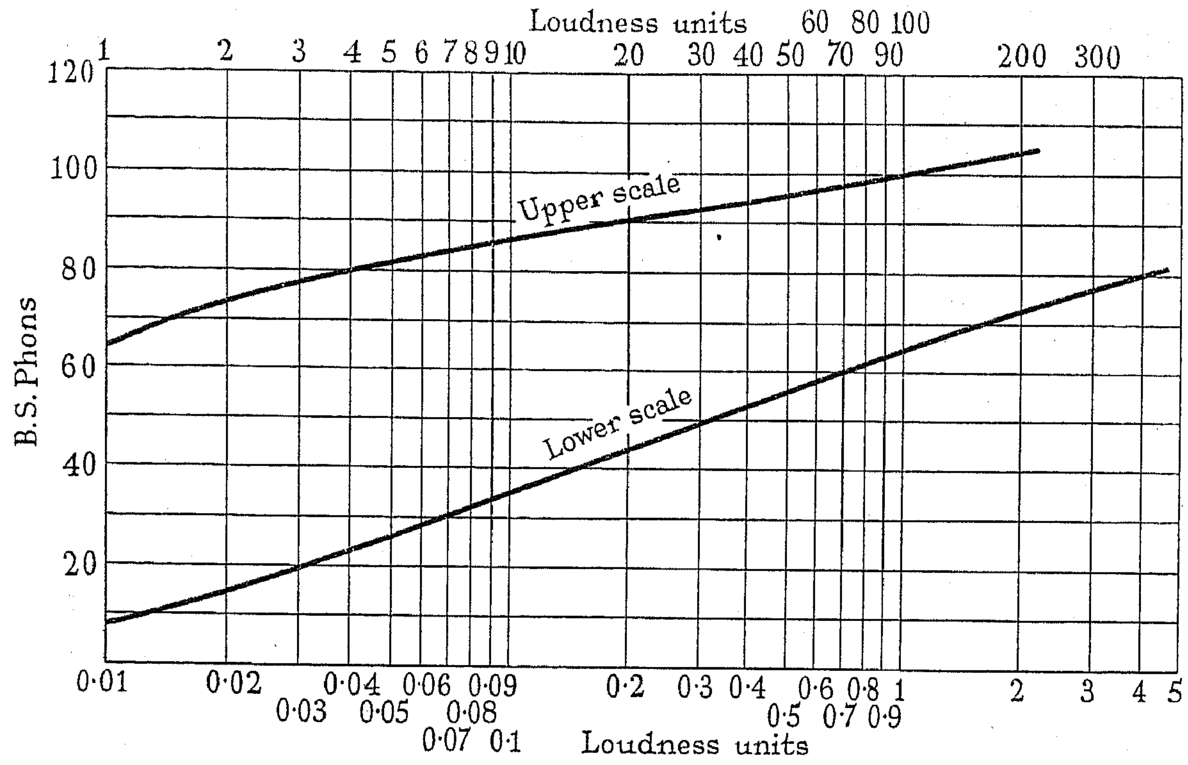

Fig. B

The authors object to the use of available objective meters because such meters are not generally equivalent-loudness meters. Noise is defined as unwanted sound: when we measure equivalent loudness we observe the sound very carefully by listening to it and by comparing it with another sound, or operating such methods of observation as lead us to determine when that sound is of equal loudness to a standard reference sound. Such deliberate observation of the sound which is a noise seems to me probably to involve different psychological reactions from those which obtain when the noise is acting wholly in its true sense of unwanted sound and preventing the observer from hearing clearly a wanted sound. I therefore find some difficulty in agreeing to the contention that an objective meter must necessarily be a true equivalent-loudness meter before consideration can be given to its adoption for general use. I feel that, while fundamental research of the type described in the paper is proceeding, the use of an agreed standard objective noise-meter, which would indeed be arbitrary but which would not be a loudness meter, might help to facilitate comparison of the results of measurements made at different times and places.

Mr. E. Tobin: Some two or three years ago the question of noise in transformers seemed to become important, and an objective instrument seemed to be the most suitable type for measuring such noises. In a factory the background noise was often fairly high, approaching the level of the transformer noise itself, and we felt we were fairly safe in calculating the equivalent energies, subtracting the energy of the background, and taking the result as the transformer-noise value. The present paper shows this method to be unsound.

On page 65 the authors refer to the theory that the loudness values of the several components of a noise are additive, and they give an example of a harmonic of 80 phons being added to a fundamental of 80 phons and giving a total equivalent loudness of 91 phons. I should like to ask whether this result was found by experiment and whether it is independent of the order of the harmonic. They also say that they conclude it is necessary to keep the largest harmonic 30 or, if possible, 40 phons below the fundamental. Can they give us any recommendation as to the maximum background level when measuring complex sound? In ordinary conditions it may not be possible to get background levels 30 or 40 phons lower than the sound which it is desired to measure. Where sound-recording apparatus is available it might be worth while to take the sound record of machinery under various excitation and load conditions, so as to have a permanent record for reference after the machine has left the factory. This record would also be useful for calibrating purposes, and for comparison with later machines.

In the case referred to in Fig. 2A the source of sound is apparently behind the noise-producing instrument. We have come across a similar case when carrying out tests on an oil-cooler with an objective instrument. The oil-cooler was about 6 ft. cube, and, taking measurements round the cooler at equal distances, we found that the intensity was the same at corresponding distances in any direction; the curve was approximately a hyperbola, and indicated that the source of sound was at about the centre of the cube; in other words, it was inside the article we were measuring and not on the surface of it.

Mr. H. Davies (*communicated*): The authors have provided considerable evidence to show that the conventional objective noise-meter does not, in fact, do what it purports to do, i.e. measure the equivalent loudness of a complex noise. It is easy to calculate from the data in Table 4 what a perfect meter having an r.m.s. summation would read. The results are given in the first column of Table B, and they show that the readings of such a meter are liable to be seriously in error.

But to say that a frequency-corrected amplifier with an r.m.s. meter does not give the correct answer is not to say that an adequate objective meter is impossible. It may be mainly that the r.m.s. summation is incorrect. Now there is already good reason to suppose from what is known about the ear that to a first approximation the summation required is one of loudness, not of energy. The authors point out that the loudness of noises A, B, C, and D, of Table 4 is concordant with this view, but it is interesting to note that the concordance goes much farther than this. It would be expected on general grounds that such a law would be correct at low intensities but would give results progressively too high as higher intensities are approached and the mutual masking of the components becomes more important.

If we apply this law of loudness summation to the data of Table 4 we find that this is exactly what happens. Up to an equivalent loudness of 80 phons the summed-loudness rule gives astonishingly accurate results, but above this level it gives figures definitely too high. Table B shows the loudnesses of the noises of Table 4 calculated on a summed-loudness basis. Column 2 is based on the loudness-intensity relation given in the authors' reference on page 65. This was determined by direct experiment, and in this particular case gives better results than a simple power law. In fact below an E.L. of 80 phons it is only once in error by more than $1\frac{1}{2}$ db. For comparison, columns 3 and 4 give the loudness calculated on the basis that loudness is proportional to the 4th and to the 5th power of the phons respectively. Although these do not give quite such good agreement, yet they do show an improvement over the conventional power summation. Noises N, P, and Q, have not been included in the table as they are at high level where the summed-loudness rule is inapplicable.

These figures give rather remarkable confirmation of the validity of the summation of loudness at low and medium intensities, but further data are required to determine the summation at higher levels. This will doubtless be a more complex matter, probably depending to some extent upon the character of the noise. However, the success of loudness summation at the lower levels suggests that an objective meter with such an amplitude characteristic would be a considerable advance on the conventional summed-power meter. The authors have performed an extremely valuable service in establishing a practical noise-measurement technique in conformity with the B.S.I. primary standard. This method, however, is somewhat of the nature of a sub-standard, and the next urgent need is for an adequate objective noise-meter.

Table B

EQUIVALENT LOUDNESS (PHONS) OF NOISES IN TABLE 4, CALCULATED ON THE BASIS OF "SUMMED ENERGY" (COL. 1) AND "SUMMED LOUDNESS" (COLS. 2, 3, AND 4)

Noise	Equivalent loudness				
		Calculated			
	Measured	Summed energy	Summed loudness		
		(1)	(2)	(3)	(4)
A	61	53	60	59	58
B	37·5	33	38	36	35
C	59	53	60	59	58
D	80	73	81	83	81
E	91	88	—	125	114
F	79	69	91·5	95	87
G	62·5	48	64	62·5	57
H	53	38	51·5	47	43
I	26	18	25	16	14
J	79·5	68	90	94	86
K	52	38	51·5	47	43
L	75	58	76	78	71·5
M	53	38	51·5	47	43
O	46	30	42	35	32

Messrs. B. G. Churcher and **A. J. King** (*in reply*): As the development of a technical subject proceeds, it is important to review from time to time the presuppositions on which it is based. Mr. West reviews the arguments which have been adduced in support of alternative bases of noise measurement. It is well to realize that the term "noise measurement" has been used with insufficient discrimination in relation to really quite separate subjects. Not long ago the assessment of electrical interference in telephone circuits, which manifests itself as noise when a receiver is connected, was referred to as noise measurement. At the present time the term is often associated with the assessment of acoustic interference with telephone conversation or sound reproduction. Here the intelligibility of a desired sound (speech or music) in the presence of a noise is the essential consideration, and it may well be that in this special subject masking is a more relevant criterion than the relative loudness of the two sounds measured separately. With the subject of acoustic interference, which is the field of the telephone or sound-reproduction specialist, the present paper makes no attempt to deal.

The more general subject of noise measurement, to which the paper relates, arises most frequently because the noise is in itself undesirable. It is true that in some cases the noise level at the listener's location may be high enough to interfere with conversation, but in the majority of cases the object in noise abatement is the ensuring of a reasonable standard of comfort, which corresponds to a level considerably below that which produces interference with ordinary conversation. Few people would tolerate in a dwelling-house, especially at night, a noise level just insufficient to interfere with telephone conversation. Hence, in noise abatement, masking is not generally the relevant criterion. Space does not permit of examples being discussed but, reviewing over a wide field of noise abatement a number of typical problems, many of which have come within our direct experience, it can be said that an essential quantity is the initial noise level, into which masking does not enter. This level, together with the level desired, enables the necessary attenuation to be ascertained. Where masking effects do enter into noise-abatement problems, they usually arise from the masking of the residual noise from the source under consideration by extraneous or background noise. In these circumstances masking is helpful if the aim is to diminish the obtrusiveness of the source and not to deal with all noise present, but in most practical cases background noise is low, variable, and uncertain, and advantage can only be taken of a minimum value. While more data on the masking of one type of sound by another are desirable, an approximate allowance for such masking as occurs may usually be made.

For the assessment of the intrinsic undesirability of a noise heard alone, we are far from suggesting that loudness is the only or final criterion. There is considerable support for the view that loudness is a most, if not the most, important criterion of undesirability. It is based expressly on the judgment of the "normal observer." Experience indicates that if persons are allowed to form their judgments naturally, without distracting or biasing influences, the group assessment of that which is understood by the word "loudness" is very definite. This may be seen from tests made with different groups of persons, given in Tables 3 and 4. That noise has other attributes besides those of pitch, timbre, and loudness, has often been suggested. Whether such an attribute as "annoyance" can be defined and assessed and, if so, what relation it bears to loudness, are certainly matters which require examination. In the meantime loudness affords, in our opinion, the most relevant basis available for noise abatement, and the B.S. Phon is essential as a standard. The loudness basis is certainly successful in dealing with practical problems of noise abatement. For acoustic interference problems a separate masking standard may be expedient. It would be better to provide such a standard than to attempt to compromise on a single basis for dealing with two really distinct questions. The separate consideration of these questions would do much, we think, to avoid the wide divergence of opinion to which Mr. West refers.

It is suggested that the paper may convey the impression that noise measurement is much easier than it really is, on account of the paper being confined to sustained noises. There are limits to what can be covered in one paper, and it is mentioned that no attempt is made to deal with unsteady sounds. We very much doubt whether the suggestion that the majority of noises fluctuate in an irregular and often violent manner is true if a sufficiently broad view of the whole field be taken. It is true that such sounds as unwanted conversation and music (which are of great importance in relation to acoustic interference), footfalls, the ticking of clocks, and the intermittent sounding of motor-horns, may not without adaptation be susceptible to measurement by the methods discussed in the paper. On the other hand, sustained noises which have to be assessed in practice arise from such sources as electric generators, motors, transformers, electricity meters, electric fans, rotary pumps, suction cleaners, electric radiators, aeroplanes, steadily running motor-cars and trains, and general machinery in which gearing is embodied. Among pulsating noises which can also be dealt with by the methods described are those from gas and oil engines, air compressors, pneumatic hammers, motor-cycles, and slowly running cars. In any case, the groundwork of noise measurement consists of the study of sustained sounds, which is a necessary preliminary to dealing with intermittent or fluctuating noises.

In his contribution Mr. West hardly refers to the principal subject of the paper, namely the feasibility of a subjective standard for loudness. To those not closely acquainted with noise measurement, his remarks may create the impression that all bases of measurement are equally arbitrary and that the choice of a standard should rest purely on convenience in use. Whatever may be the case in respect of the subject of telephone acoustic interference, this impression is not well founded in the case of the more general subject of noise abatement.

In reply to Mr. Mason's comments on the number of observers needed for consistent results in subjective measurements, the basic technique described in Section (3) is the best in this respect, but it is, of course, intended as a primary laboratory standard only, for use in the calibration of portable meters and other fundamental measurements. Of the portable secondary subjective meters, the paper shows that the 2-telephone meter is more consistent than the 1-telephone type, for reasons which are suggested. We fully agree that subjective methods have serious disadvantages for some practical measurements and that the objective type of meter is desirable for most purposes. The distinction between loudness and equivalent loudness is mentioned on page 57 of the paper. References to papers dealing with the relation between the two are given. As the relation may be expressed by a smooth curve, it is not correct to say that there is no simple relation. This relation has, of course, nothing to do with the discrepancies of objective meter readings with different types of noise, and the selection of the calibration curve for a particular type of noise would not be of much help in this direction. To be effective, a standard must constitute the sole standard. The use of the word "phon" for another quantity would be most undesirable. Mr. Mason somewhat exaggerates the point we make regarding the directional properties of the microphones of objective meters. The use of non-directional microphones would be entirely appropriate if persons were provided with non-directional heads. We agree that the possibility of errors due to background noise is brought more immediately to the notice of the observer with subjective than with objective meters. It is desirable that background noise should be at least 15 phons below the noise under measurement. Whenever it is at all possible, we make a practice of choosing a time when this condition obtains in such cases as factory or outdoor measurements. Caution should be exercised in attempting to eliminate background noise effects by mentally segregating the two noises.

We are much indebted to Dr. Fisher for his comments on the statistical methods we have used for the basic measurements, a subject on which he speaks with the authority of wide experience. As indicated in the paper, the existence of two components of dispersion, which we refer to as the casual and systematic divergencies, have made us doubt whether the methods we have used in estimating the probable error are entirely satisfactory. However, we are glad that Dr. Fisher confirms that elaborate methods are not needed with the number of observers which is sufficient to yield the precision desired. We appreciate his suggestions towards simplified statistical procedure and have already adopted them for later work. We are obliged to him for pointing out the mistake in Yule's book. It is fortunate that it leads to no error in practice.

Dr. Billig refers to terminological confusion, and in this connection we would again take the opportunity of commending to the notice of those who have occasion to deal with acoustics, the British Glossary of Acoustical Terms and Definitions, B.S.S.661—1936. We agree that discrimination between equivalent loudness and loudness is long overdue, but we can hardly agree that the two terms have nothing to do with each other. References to papers discussing the relation between them are given on page 57 of the present paper. It is true that E.L. is stated in terms of the physical intensity of a standard tone, but the equality of the loudness sensation the tone evokes with that evoked by another sound is essentially a matter of personal impression. We agree that the most fundamental scale for reference tone magnitudes is a simple pressure scale, but, although it is now generally appreciated that the Weber–Fechner law does not apply to loudness, a logarithmic scale is to be commended for ordinary purposes as being mathematically and instrumentally convenient. As to the difficulty of comparing a noise with a pure tone, the paper shows that if the normal mode of listening can be chosen, as in basic laboratory measurements, the comparison itself is much easier than is generally supposed. It should be realized that impressions of the inconsistencies of subjective measurements have been derived almost wholly from the use of portable meters, involving telephones, monaural listening, the adjustment of dials, unsteady sounds, extraneous noise, and other abnormal or distracting factors. The real difficulty arises when we attempt to construct an objective meter to perform, in effect, the comparison. We fully agree that the use of objective meters is essential for many practical purposes, but not that what we have referred to as the conventional objective meter is the only feasible type.

In reply to Mr. Hutchinson's remarks regarding Table 7, we would point out that the mean value for any one of the four tests, involving 5 readings only, was sufficient to obtain a value to within 1 phon of the basic value of 62·5 phons. It was not to be expected from the title of the paper that we had dealt fully with the design of an adequate objective noise meter. While agreeing as to the pressing need for such a matter, it is first necessary to explore the basis on which the performance of such meters is to be appraised, and this has been the main object of the paper.

Our statement regarding Fig. 7, viz. that the error of the objective meter decreases at high and low intensities, is on a basis of equivalent loudness and not on a percentage basis.

Mr. Gates raises the question of the relative aural sensitivities of men and women. It would be inadvisable to attempt any generalization from so few tests, but we have not found any marked difference in their capacities to judge equality of loudness or in their equal-loudness judgments. The effect of fatigue in increasing the dispersion of psychophysical measurements is well known. General information on the effect of sound-proofing measures is best obtained by tests with single pure tones, in which case an objective meter of known response to pure tones is very useful.

Mr. Gates's suggestion to use a series of octave filters has already been tried in Germany* but, as he says, errors arise due to two or more components occurring in an octave. However, his summation curve, Fig. B, derived from our Table 4, is very interesting as the agreement between the calculated and measured values shown in Table A is as close as can be expected. The range of noises covered is considerable, from 103 down to 26 phons, and we have also tried it with other noises of known composition. The relation therefore has some claims to general validity, but it fails in some cases, like the one quoted by Mr. Hutchinson, because it takes no account of frequency separation. We are not surprised that Mr. Gates's Fig. B differs considerably from the 5th-power loudness curve in Fig. 3 (Ref. 2), since they refer to different conditions. The 5th-power law refer to the loudness of sounds heard one at a time, while Fig. B applies to sounds of the same order of loudness heard simultaneously.

In reply to Mr. McMillan, the desirability of an objective meter, the performance of which, referred to the phon standard, is independent of the type and level of noise over a wide range, is so obvious that we are surprised it should be questioned. Our analogy of an a.c. voltmeter seems to us an apt one, but regarding both instruments we say they should read correctly " over a wide range," not in every conceivable case. Mr. McMillan's difficulty arises when it is desired to obtain a measure of the aural importance of the constituent voltages, but this is not measuring voltage.

His question about the relevance of the E.L. of a noise to its undesirability is dealt with in the reply to Mr.

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* H. G. Thilo and U. Steudel: *Wissenschaft Veröffentlichungen auf dem Siemens-Konzern,* 1935, No. 14, p. 78.
E. Freystedt: *Zeitschrift für Technische Physik,* 1935, vol. 16, p. 533.

West. We agree regarding the usefulness of the objective meter, providing it is calibrated in subjective terms. The desire for an instrument reading should not make one forget that the ear is the final arbiter and that a meter which conflicts with it can be misleading instead of helpful.

Mr. Tobin's question about the permissible background noise during a noise measurement is dealt with in the reply to Mr. Mason. We state on page 62 that the virtual source is approximately 2 m. behind the opening of the horn loud-speaker.

The summed energy figures given by Mr. Davies in Column 1 of Table B are given in the third column of our Table 10, along with the observed values in some cases and errors in subsequent columns. The summed loudness figures which he gives are of considerable interest.

SINGLE-PHASE ELECTRIFICATION ON THE PENNSYLVANIA RAILROAD*

By H. C. GRIFFITH.

(*Paper received 2nd November, 1936.*)

SUMMARY

This paper describes the steps leading up to the present electrification of the Pennsylvania Railroad, the system used, the extent, and the operating results. The 4-track main line is now electrified between New York, Philadelphia, Wilmington, Baltimore, and Washington, as are also the commutation lines around Philadelphia and New York.

Since this paper was prepared, work has started on the extension of the electrification to include the connecting lines for both passenger and freight service west to Harrisburg, Pennsylvania. The chief parts of lines in the new electrification are the main line from Paoli, just west of Philadelphia, through Lancaster to Harrisburg; the low-grade freight line from Morrisville, Pennsylvania, near Trenton, New Jersey, via Columbia to Enola Yards, near Harrisburg; the freight line from Columbia following the Susquehanna River to Perryville, Maryland; the freight line from Monmouth Junction to South Amboy, New Jersey, with necessary yards, connecting branches, and equipment.

The new work will involve the electrification of 315 miles of line and 773 miles of track. Upon its completion the Pennsylvania Railroad system will have 41 per cent of the total electrically-operated standard railroad track in the entire United States.

INTRODUCTION

The introduction of the single-phase 11 000-volt overhead catenary system of electric traction on the Pennsylvania Railroad took place in 1915, when the first step, from Philadelphia to Paoli, of the suburban electrification around Philadelphia was placed in service. Prior to that installation the third-rail 650-volt direct-current system had been used on three previous electrification projects on the Railroad and its associated lines.

EARLY STEPS IN ELECTRIFICATION

The first of these third-rail installations was the suburban electrification in 1905 of the Long Island Railroad from its Brooklyn terminal to the residential sections of Long Island. The initial installation comprised 44 route miles and 110 miles of track. The reasons behind the use of electric power on the Long Island line were the growth of traffic, making old facilities inadequate; and legislative action, limiting the use of steam locomotives.

In 1906 a similar system of electrification was carried out on the West Jersey and Seashore Railroad on its line through southern New Jersey from Camden to Atlantic City. The object of changing this line to electric power was to try out the traffic-building ability and the economy of operation of electric traction over a line with both express and local passenger business.

In 1871 the Pennsylvania Railroad secured an entrance to the New York district by lease of the United New Jersey Railroad and Canal Co.'s line which terminated in Jersey City and connected to New York by ferry. The management from the beginning endeavoured to find a better way of delivering passengers to New York. Both bridges and tunnels were projected. The advantages of a tunnel were apparent, but until the advent of electric traction a tunnel was not feasible because of the length underground and the unavoidable grades.

When the Pennsylvania Railroad obtained control of the Long Island Railroad in 1900, each of the two companies had been working on plans for new terminal development. These plans finally resulted in a joint terminal at 32nd Street and Seventh Avenue, New York. Plans for the New York tunnels were pushed forward after it had been determined that electrification was feasible; the necessary legislation was passed in 1902, and construction was commenced in 1903.

The management decided to make the section of the tunnels such that either the low-voltage third-rail or the high-voltage catenary system could be installed, holding back the choice of the system until the time came to start the electrical work. In the meantime a test track was equipped with 11 000 volts on the catenary and an a.c. locomotive was built. Extensive tests were conducted with experimental sections of tube simulating the tunnels. As a result of the tests and in view of the successful operation of the electrification on the Long Island Railroad, it was decided to continue the use of the third-rail system with direct current at low voltage for the terminal developments. In 1909 experimental d.c. locomotives were constructed to perfect the type to be used, and in 1910 the momentous terminal development was opened to the public.

The tracks east from the station in New York are used by both Pennsylvania and Long Island trains and extend through four tubes under the East River to Long Island. The majority of the Pennsylvania trains continue through these tunnels and either terminate in the large coachyard on Long Island or continue on over the Hell Gate Bridge, completed in 1917, back to the mainland to connect with the New York, New Haven and Hartford Railroad to New England. The Pennsylvania trains from the south and west leave the old main line to Jersey City at a point where a new station was established with the name of Manhattan Transfer. Here the steam locomotives were changed for electric locomotives. The trains then sweep across the Hackensack Meadows on a two-track right-of-way to the Bergen Hills, where they enter the tunnels. The two parallel tunnels pass under Weehawken and then below the Hudson River and into the station in New York.

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* The Papers Committee invite written communications, for consideration with a view to publication, on papers published in the *Journal* without being read at a meeting. Communications (except those from abroad) should reach the Secretary of The Institution not later than one month after publication of the paper to which they relate.

The Long Island Railroad extended its d.c. electrification several times and now has 130 miles of route and 350 miles of track, electrified with the d.c. third-rail system for suburban service. The Pennsylvania Railroad did not extend its d.c. electrification, although excellent service had been obtained with it during the 22 years for which it was operated.

INTRODUCTION OF SINGLE-PHASE SYSTEM

The a.c. system had shown such promise when tested in 1908 that it was determined that the system should be tried out further in actual service to determine its suitability. Accordingly, when it became necessary in order to relieve congestion to electrify the suburban service of the Philadelphia terminal area, the single-phase system was utilized. The principal station, known as Broad Street Station, was a stub-end terminal handling heavy main-line and commuter traffic. By the use of multiple-unit cars the number of train movements in the station area could be greatly reduced, with a corresponding increase in the capacity of the existing station facilities. The first electrification of the suburban lines out of Philadelphia was completed in 1915, using the single-phase 25-cycle 11 000-volt system, and extended on the main line to the west as far as Paoli for a distance of 20 miles. Following this, the line to Germantown and Chestnut Hill was electrified in 1918, the branch to Whitemarsh in 1924, south on the main line to Wilmington and the branch to West Chester in 1928, and east on the main line to New York as far as Trenton and the branch to Norristown in 1930. This completed the electrification of the suburban service in the Philadelphia area. Coincident with the inauguration of electric service on the last lines in 1930, new suburban passenger stations were opened at West Philadelphia and Broad Street. The latter is an underground station located and designed in an advantageous manner such as would not have been possible except with electrification.

THROUGH ELECTRIFICATION

As far back as 1913 the Pennsylvania Railroad had made studies of the problem of electrifying various sections of the railroad for through-traffic movement. In 1927 a report was presented to the management which developed the cost of, the savings due to, and the increased capacity resulting from, electrifying the line between New York, Philadelphia, Baltimore, and Washington along the eastern seaboard, and west of Philadelphia including important branches to the Susquehanna River. The report indicated that adequate savings could be realized from the electrification of this integrated area, with corresponding simplification of the general operation of the railroad service. In 1928 came the announcement of a programme of electrification to include the territory beginning at the Hell Gate Bridge, New York, where connection is made with New England, and extending west and south to Wilmington, west from Philadelphia on the main line in the direction of Harrisburg as far as Atglen, Pa., and the low-grade freight lines which join at Columbia, Pa., and connect the cities of New York, Philadelphia, and Wilmington with the West. The project was to be undertaken progressively in sections and to extend over a period of several years.

The immediate factors which influenced the decision to proceed with electrification at that time were: (1) The greater economy of electric traction as compared with steam operation in dense traffic territories. (2) The growth of the southern passenger business. (3) The increasing density of both freight and passenger business on the eastern section, and the probability that in the future more rapid movement would be required. (4) The desirability of utilizing the advantages of electric traction in the construction of new passenger stations at Philadelphia and Newark. (5) The desirability of building locomotives which would meet the requirements of the territory for the future as regards train weight, speed, and reliability. (6) The probability that the project could be completed at a less expenditure than if started at a later date.

In October, 1929, negotiations extending over several years were concluded between the Railroad and the City of Baltimore for the improvement of the main line through that city. The Railroad immediately announced the expansion of its electrification, by that time under way, to include the line from Wilmington to Baltimore and Washington for passenger service and thence on to Potomac Yard near Alexandria, Virginia, for freight service.

The suburban electrification around Philadelphia had involved three sections of main line which would naturally become sections of the through electrification. The first, the Paoli line toward the west, was constructed prior to a complete study of the through electrification. Here a relatively low transmission voltage of 44 000 volts was used. This installation provided, however, a field for experiments with an eye to future installations. Many design changes in catenary construction, in substation apparatus, and in motor, car, and locomotive equipment, were tried out. In 1928, when the Wilmington suburban service on the main line to the south was electrified, many changes were introduced. The transmission voltage was increased to 132 000 volts with the possibility of through electrification in mind.

The final step in the suburban electrification out of Philadelphia, and also the first definite step after the programme of through electrification had been announced, was the electrification of the main line north and east out of Philadelphia as far as Trenton for suburban service. The 132 000-volt transmission system which had been used to Wilmington was extended to Trenton, and the latest designs embodying still further improvements were used.

Difficulties of Construction

As the suburban electrification around Philadelphia neared completion in the latter part of 1929, the construction of the through electrification between New York and Washington was started. The first section of the project placed under construction was that between Sunnyside Yard, Long Island, and Manhattan Transfer, New Jersey, where the third-rail electrification had been in service some 20 years. This portion of route presented many engineering and constructional difficulties and it was thought wise to start the work at this most difficult point, that ample experience might be secured from its operation while construction work on the balance of the project progressed.

In this section of 13 route miles are 85 miles of track (of which almost one-quarter is in tunnels or under ground), the network of tracks and facilities comprising Sunnyside Yard, the busy passenger terminal at Pennsylvania Station, and a traffic density so great as to require much of the work to be done at night—in some areas for periods of only 6 hours out of the 24.

In addition to this, many of the tracks were to be used by both alternating-current rolling stock and direct-current rolling stock, necessitating careful engineering study to ensure that the design of alternating-current equipment provided properly for the presence of large quantities of direct current and that the direct-current equipment already installed be properly protected against injury because of the presence of alternating current. The presence of both alternating and direct current in large volume in the track rails, and the passage of direct current through the transformers on locomotives and cars and substations, also required careful check of signal circuits and equipment to ensure that the fourth harmonic present on the 25-cycle current waves did not combine with the currents of the signal system, of a nominal frequency of 100 cycles per sec., to produce improper signal action.

Other problems existed. The track in all tunnels had been in use for 20 years and the depth of ballast under the ties had gradually increased until in many places the distance between equipment roof and tunnel roof was insufficient to permit the installation of overhead trolley construction. This necessitated lowering long stretches of track to its original position, a process accompanied by considerable difficulty under the heavy traffic conditions existing in these tunnels. It was necessary to design and install, in the 18-in. space between equipment and tunnel roof, a trolley system suitable for high-speed service, carrying 11 000 volts to earth, as well as to provide sufficient clearance between the live parts and the earthed car and tunnel roofs to ensure reliable operation. In the land portion of the tunnels considerable seepage existed through the roof and this had to be corrected within the area above the pantograph shoe, to avoid formation of icicles with the resulting short-circuits and breakage of pantographs in cold weather.

Other problems both of engineering and of construction existed, but through the close co-operation of engineers and manufacturers all were successfully met; and in January, 1932, 2 years after work had been started, single-phase alternating-current operation was substituted for direct-current operation between Sunnyside Yard and Manhattan Transfer.

New York–Philadelphia Section

Meanwhile work was progressing between Manhattan Transfer and Philadelphia. In January, 1933, electric operation of the through passenger service was inaugurated between New York and Philadelphia, and in March and April of the same year electric operation of southern trains to Wilmington and western trains to Paoli was started. At these points the electric and steam locomotives were interchanged.

Wilmington–Washington Section

In December, 1933, final arrangements were made to finance the completion of the electrification from Wilmington to Washington as well as the additional electrification between Wilmington and New York needed for freight operation.

Preliminary Work

At this time the engineering designs for the balance of the project were well under way and progressing slowly. Construction work, however, was completely shut down and only the nucleus of a small force was being retained by the Railroad Company. A careful estimate of the time necessary for completing the electrification was made, and January, 1935, was decided upon as the date for beginning passenger service and June, 1935, for freight service, though several major problems had yet to be solved.

A construction force of 11 000 men, drawn from the ranks of furloughed employees of the Railroad, was to be built up to apply the electrification to the Railroad. The necessary work train and other equipment for the use of these men had to be provided, the engineering designs completed at a rate sufficient to keep ahead of the construction force, and the materials supplied at a rate necessary to complete the project on time. To meet these requirements, 54 sets of work-train equipment, 220 trucks and passenger automobiles, and 4 fully-equipped storehouses were necessary, apart from the engineering designs, materials, and equipment; the latter in many cases could not be provided until after the engineering designs had been completed.

Special Problems of the Project

One of the difficulties of the extension of the electrification was that engineering problems arose which had to be solved rapidly, in order not to delay the programme.

Within the cities of Washington and Baltimore, the 132 000-volt transmission circuits of the Railroad are run in underground cable. Experience with underground cable for circuits of this voltage is not very extensive, and so, to secure such experience, two different types of construction were adopted. The engineering investigation and design work in connection with these installations was of such a character that it had to be done thoroughly rather than hastily, and it was necessary to plan out this work carefully in order that both engineering and constructional operations might progress as rapidly as possible, consistent with the securing of a first-class installation.

Another special problem was the existing railroad tunnel at Baltimore, nearly 8 000 ft. long, through which passes a heavy passenger and freight service. The application of the electrification circuits to this section required the most careful co-ordination of the work-train service to avoid delays to passenger trains and at the same time to give the maximum use of track to the work trains. The engineering design of the catenary supports was worked out to give the maximum trolley-wire height possible consistent with the restricted clearance in the tunnel, to permit the movement of freight trains with lading of unusual height.

Through the Baltimore Station area the track arrangement was not so satisfactory as could have been desired, and, in connection with the electrification work and the building of the new Union tunnel north of the city, it was decided to rearrange these tracks so as to make better provision for the through freight and passenger movements, and also to lower them through the station proper and thus provide adequate headroom for the overhead trolley wire. The work of rearranging these tracks was not under the direction of the electrification organization but was carried out simultaneously with the electrification, and close co-ordination was necessary between the two projects.

Throughout the territory to be electrified considerable preparatory work had to be done before the electrification construction work could be started. Some of this work had already been completed when the electrification construction was authorized in December, 1933. That still remaining to be done included the placing of communication and signal control lines under ground to ensure their protection against severe weather conditions, as well as to clear the right-of-way for the electrification circuits; the provision of proper overhead clearances for trolley-wire circuits where overhead construction such as highway bridges and tunnels existed; the removal or proper re-location of wire crossings of other ownership than the Railroad to provide proper transmission- and trolley-circuit clearances; the re-location of the signals to coincide with the pole locations for the overhead trolley system; and the changes in signals, where necessary, to enable their circuits to co-ordinate properly with those of the trolley system.

At a number of points drawbridges existed. It was necessary to provide continuous trolley-wire contact, suitable for high-speed service, across the draw spans, and at the same time to permit opening of the bridge. Each bridge was different in type or construction, and required individual treatment.

There were also several crossings at grade with trolley-cars. It was necessary to provide continuous power supply to the locomotives at 11 000 volts alternating current, and to the trolley-cars at much lower d.c. voltages, and at the same time to keep the two systems isolated from each other. One of the most interesting and ingenious devices consisted of a trough for the car trolley wire at a lower elevation than the a.c. contact wires, which could be rotated about a vertical axis to connect the ends of the car trolley wire on each side of the railroad when a car was passing; but otherwise remained parallel to, and between, the railroad tracks and clear of passing trains.

At Washington, the freight line passes through part of the city's park system and then crosses the Potomac River over a long bridge. Special attention was paid to the appearance of the structures within the park area. Two 11 000-volt trolley feeders were carried across the river in concentric submarine cables and continued underground through the park area as standard cable to avoid additional overhead wires. The concentric submarine cable which was employed was new in type and required special design and handling. The route of nearly 4 000 ft. of this same line in Washington is through the Virginia Avenue tunnel, where the two tracks had to be replaced by one track and the grade lowered 2 ft. to provide trolley clearance. Extensive repairs to concrete and drainage were also needed. Throughout this work one track had to be always available for the frequent freight service in both directions, but careful planning and attention to detail accomplished the desired result.

POWER SUPPLIES

General

While the power supplies had already been arranged for, additional details as to the exact location and design of step-up stations at supply points had to be decided. Power is supplied from seven sources, as follows: (1) The Railroad Company's power plant in Long Island City, New York. (2) A frequency-changer station of the Public Service Electric and Gas Co., at Metuchen, New Jersey. (3) and (4) Two frequency-changer stations of the Philadelphia Electric Co., one at Richmond, Philadelphia, and the other at Chester, Pa. (5) An emergency supply from the Schuylkill station of the Philadelphia Electric Co. in Philadelphia. (6) and (7) Two supplies from the Aldred Companies of Baltimore (Consolidated Gas, Electric Light and Power Co., Pennsylvania Water and Power Co., and the Safe Harbor Water Power Corporation), one at Perryville, Md., and the other at Benning, Washington, the latter through the system facilities of the Potomac Electric Power Co.

This arrangement adequately protects the service by providing power supplies at points well distributed along the route, and at the important Washington and New York terminals, while at the same time it permits the supply of blocks of power from each source of sufficient volume to ensure economy of production.

Power-Supply Stations and Substations

The transformer capacity installed at the supply points totals 452 500 kVA (continuous rating) and, except for the supply from the Railroad Company's own power plant in Long Island City and one 25-cycle waterwheel generator at the Safe Harbor power plant, this power is obtained from the supplying companies through 60/25-cycle frequency-changer motor-generator sets from interconnected 60-cycle power-company systems. These supplies are normally operated in parallel through the Railroad's 25-cycle transmission lines as well as through the power companies' own 60-cycle systems and are split apart only when the 60-cycle systems are out of step, or if there is a possibility that this eventuality may occur because of lightning storms or similar emergencies.

At the principal supply stations, cables from the 25-cycle generators connect through circuit breakers to a nominally 13 200-volt tubular ring busbar divided into a number of sections by means of busbar tie circuit breakers. From these sections feeds are taken through circuit breakers to the transformers. The transformers themselves are connected direct to the transmission lines through horn-gap sectionalizing switches. Oil circuit-breakers on the ring busbar in these supply stations have a continuous rating of 5 000 amperes and an interrupting rating of 120 000 amperes.

At the Railroad Company's Long Island City power plant, transformers of approximately 1 : 1 ratio are used in order to isolate the generator at this point from the trolley-rail system, and these transformers feed into the underground concentric-cable distribution system in the New York terminal area, at 11 000 volts. This system is in turn connected to the 132 000-volt transmission system through transformers at Hackensack Portal substation on the New Jersey side.

The remaining supply stations feed directly into the 132 000-volt transmission line at the supply stations through transformers of 15 000 or 20 000 kVA continuous rating.

A total of 11 generators are connected to the system, with a total generating capacity of 317 320 kVA (continuous rating), or 234 950 kW at the power-factor rating of the various machines.

Step-down substations tap the transmission line at intervals of approximately 8 miles. Over the 373 miles of route, included in which is the main line from Philadelphia to Paoli and various branches, as well as the main line between New York and Washington, are located a total of 40 transformer substations, with 152 transformers, having a total continuous rating of 659 000 kVA and including 26 000 kVA of step-up transformers which feed the 44 000-volt circuits from the 11 000-volt busbars at two stations. The present design of substation is of the outdoor type, contrasting with the totally enclosed indoor type of 20–30 years ago. The transformers are of the oil-insulated, self-cooled type, instead of the air-cooled, and oil-insulated, water-cooled types used in the early substations. A transformer size of 4 500 kVA continuous rating has been adopted as standard. A total of 134 units of this size are installed. These transformers have ample overload capacity and have been carefully co-ordinated both with respect to bushing and as regards internal insulation with the transmission line so as to eliminate, as far as modern design permits, any possible failure due to excessive voltage. The older protection schemes using choke coils and electrolytic lightning arresters have been replaced by co-ordinating gaps and arcing rings or modern lightning arresters. The later transformers have a high impulse-voltage rating, permitted by modern design knowledge.

High-voltage circuit breakers are not used in the modern substations for transformer switching. The transformers are considered a part of the transmission line, separated only by horn-gap sectionalizing switches. Circuit breakers in the 132 000-volt transmission line are installed at two points only, at Zoo substation, Philadelphia, and at Perryville substation, in Maryland, where they are used to split the transmission system into three parts should the power-supply stations become cut apart from their interconnections.

One of the most valuable developments in substation distribution apparatus is the high-speed trolley circuit-breaker. Formerly trolley circuits were supplied through standard-type oil circuit-breakers operating at a comparatively low speed, and because of this low speed the occurrence of a short-circuit sometimes resulted in burning down the trolley wire. The present-day trolley circuit-breakers contain little or no oil and are capable of interrupting a short-circuit of 50 000 amperes in less than 1 cycle on a 25-cycle basis, or approximately 0·04 sec. Frequently it is not possible to locate the exact place where the fault occurred, so quickly has it been cleared. There are 395 of these high-speed circuit breakers in service, feeding the trolley circuits of the electrification.

In addition to the transformer stations feeding the main-line trolleys, eight trolley switching stations have been installed in the yard and terminal areas. The large number of circuits in these areas has resulted in the adoption of a scheme in which a high-speed circuit breaker feeds a busbar from which a number of sections are each fed by a circuit breaker of low interrupting capacity, suitable only for handling load currents. In the event of a fault on one of the sections the high-speed circuit breaker opens and, after the circuit has become de-energized, the low-interrupting-capacity circuit breaker opens the section at fault; following this the "master" circuit breaker recloses, restoring power to the trolley sections unaffected by the fault. A total of 81 of these low-interrupting-capacity circuit breakers have been installed.

TRANSMISSION SYSTEM AND STRUCTURES

Overhead Lines

The transmission lines and signal lines are in general carried on the structures which support the catenary contact system. These structures are spaced approximately 20 per mile, and the usual construction is divided among three different types.

The first of these is the cross catenary structure. This consists of the poles with their foundations and guys; the cross catenary span, which supports the weight of the longitudinal wires over the track; the body span, which is the horizontal member just below the cross catenary wire and which takes the side load in the main messenger; and in most cases a steady span, which prevents excessive lateral movement of the contact wire. The poles are of the " H " beam type, although in the earlier installations tubular poles were used. All of the cross wires are of stranded bronze, the steady span being of $\frac{7}{16}$ in. diameter and the others varying from $\frac{5}{8}$ in. to 1 in., depending upon the loads to be supported. All these strands come cut to length and equipped with swaged or drawn fittings on each end. Adjustments are made with turnbuckles. The structure guys are of galvanized extra-high-strength steel strand and vary in diameter from $\frac{7}{8}$ in. to $1\frac{1}{8}$ in., depending upon the loads. This general type of construction has many advantages, such as less cost and somewhat better visibility along the tracks, and is used where right-of-way conditions permit.

The second type utilizes the beam form of structure. The poles are not guyed and the cross spans are replaced by a cross beam, which, in turn, is stiffened by sag braces. The insulators for the main catenary are suspended from the beam. Generally a steady span is used with this structure.

A third and less frequently used type utilizes the bracket structure. This is a single guyed pole supporting a bracket, which in turn supports the main catenary wires of one or two tracks.

A unique feature of these general types of construction is extreme longitudinal and torsional flexibility combined with a high degree of transverse strength. At the top of the poles on each side of the right-of-way is a 4/0 A.W.G. (American Wire Gauge) 7-strand copper static wire (see Figure on page 97), with bronze strand substituted at locations where greater longitudinal strength is desired. The transmission conductors are supported about 10 ft. below the static wire by 11 insulator units of the standard cap-and-pin design, 10 in. diameter by $5\frac{3}{4}$ in. spacing, having a specified mechanical and electrical test strength of 11 000 lb. The potential is 132 000 volts between conductors and normally 66 000 volts to earth, the mid-point of the step-up transformers being earthed through a 330-ohm resistor at the supply points. Between Philadelphia and New York there are four such circuits, two on each side of the tracks. The configuration is vertical with 14 ft. vertical separation between conductors and 17 ft. horizontal separation between the two circuits on the same pole. In the territory between Philadelphia and Washington there are two circuits, one on each side of the right-of-way. Here the configuration is horizontal, the conductors being separated by 17 ft. All circuits are transposed midway between substations.

The conductors consist either of hollow copper or of steel-reinforced aluminium (A.C.S.R.), about equal amounts of each being used. Four designs of hollow copper wire were installed, all having a cross-sectional area of 250 000 circular mils (c.m.) and all except one having an outside diameter of 0·731 in.; the diameter of the fourth type is 0·818 in. A.C.S.R. of 477 000 c.m. size is used; this conductor has a diameter of 0·858 in. All transmission and static wires are strung with a sag of 5 ft. in a 300-ft. ruling span. This results in a normal tension of 1 845 lb. for the hollow copper and 1 470 lb. for the A.C.S.R.

The original transmission circuits on the Paoli and Chestnut Hill electrifications, constructed before the through-electrification scheme was carried out, are of lower voltage. These circuits operate at 44 000 volts and are supported on pin-type insulators. No. 2/0 A.W.G. copper was used on the Paoli circuits and 2/0 A.W.G. equivalent A.C.S.R. on the Chestnut Hill line. These circuits are spaced 5 ft. horizontally and 3 ft. $2\frac{1}{2}$ in. vertically. They are, of course, still in service.

The signal power transmission system on the electrified tracks consists of a single-phase 100-cycle 6 600-volt circuit carried on the catenary poles at about the elevation of the body span. The insulators are 27 000-volt, pin type, and the No. 1/0 A.W.G. copper conductors are spaced 7 ft. horizontally. This circuit is tapped through transformers at signal bridges and other locations where a supply of energy is required for signal operation. It furnishes a certain amount of energy for station lighting, highway crossing signals, etc.

Cable Installations

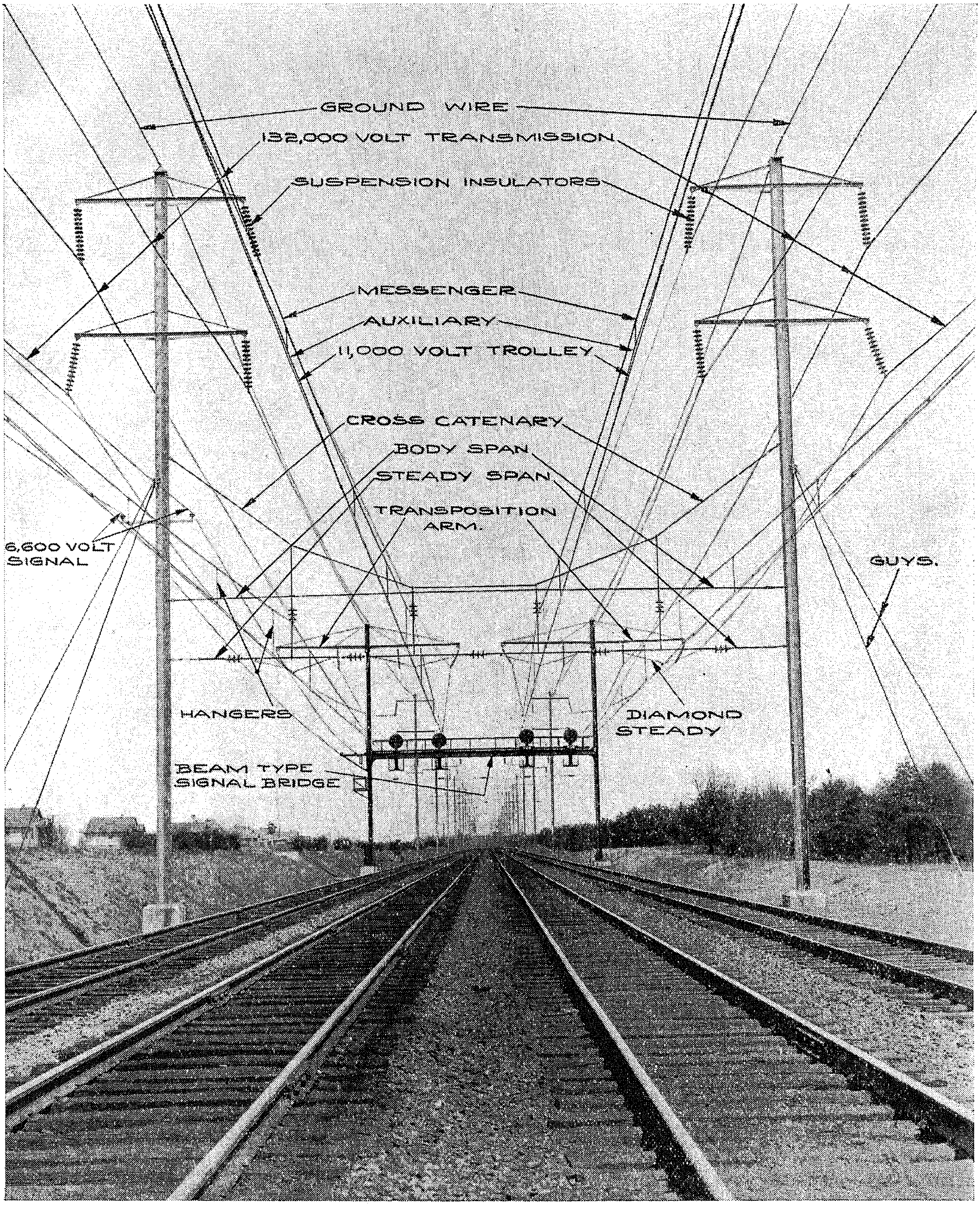
Through the cities of Baltimore and Washington it was not practicable to use overhead construction for the 132 000-volt transmission circuits and it was therefore necessary to utilize cable construction. Two types of cable were available for such a voltage and it was decided that in order to obtain wider experience, as well as to provide additional safeguards to the service, both types should be used.

Through Baltimore the "Oilostatic" system was installed. This high-voltage cable system is formed by pulling the 132 000-volt insulated cables into a steel pipe and, with the cables in place and spliced, filling the pipes with insulating oil under 200 lb. per sq. in. pressure. Two paper-insulated 500 000-c.m. cables forming the two sides of the single-phase circuit are used in a steel pipe of $6\frac{1}{2}$ in. inside diameter. This system uses the same thickness of insulation as is used in hollow-core oil-filled cables. Shielding tapes are wrapped over the insulation, and over the shielding tape is applied a canvas tape which acts as protection when the cable is drawn into the pipe. The cable is impregnated in a similar manner to that of solid-type cable, and then a thin lead sheath is applied. This lead sheath is removed when the cable is drawn into the pipe. For the longer lengths, the longest being 3 800 ft., copper armour wire is placed over the insulation for distributing the high pulling stresses which occur during installation.

The cable pipes are sectionalized by means of semi-stop joints and terminate in a manifold with each insulated conductor extending upward through its own riser pipe to terminate in a separate pothead. In the potheads a specially designed thick inner porcelain is placed around the cable to carry the 200 lb. per sq. in. oil pressure. An outer porcelain shell is placed over all, and the space between the two tubes is filled with oil under atmospheric pressure.

This construction extends from the north portal of the Union Tunnel at Bond Street to the south portal of the Baltimore and Potomac Tunnel at Fulton Junction, a distance of 3·13 miles, with two circuits throughout. In the older tunnel, the cable system was surrounded by a reinforced-concrete envelope built into the side-wall of the tunnel at about the elevation of the rails. This envelope serves to protect the cable system in case of derailments. In the new Union tunnel a prepared trough was built into the elevated walkway through the tunnel in which the pipes were laid. They were then covered with a layer of sand, and a thin layer of concrete was placed on top to complete the walkway. Outside the tunnel the pipes were given a coating of bitumastic material and buried directly in the ground. The cables from both directions are terminated in the Baltimore substation, which is located between the two tunnels. Here are also located the necessary oil-treating and pressure-maintaining equipment.

In Washington the more familiar Pirelli type of 132 000-volt cable is used. This cable utilizes a lead sheath to contain the insulating oil, and a hollow conductor furnishes a centre circulating duct for the oil. A nominal pressure of 5 to 10 lb. per sq. in. is maintained by the use of suitable reservoirs. The cable has a cross-section of 450 000 c.m. It is installed generally in 4-in. fibre ducts encased in concrete in the manner commonly used for lead-covered cables, excepting through the Virginia Avenue Tunnel where slots were provided along the wall of the tunnel and the cable installed in them and covered with pre-cast concrete blocks. This type of construction was used from a point just south of the Anacostia River through the tunnel to the substation



Typical four-track construction on the Pennsylvania Railroad.

Four 132-kV transmission circuits are shown, with guyed cross-catenary structures and a signal bridge in the background designed to act as a supporting structure for the catenary and transmission lines.

at South Capitol Street in Washington, a distance of approximately 2 miles, with two circuits throughout.

Both types of cables are directly connected from the potheads to the overhead lines through horn-gap switches.

DISTRIBUTION SYSTEM

In discussing the distribution system, let us consider first the rails. These are like any other rails of equal weight and section, but they are of interest because they become a part of the traction circuit. Of principal interest is the method of bonding: after considerable research work extending over many years a standard bond was adopted. This consists of two parallel strands each made up of 7 steel and 10 copper wires, with the ends butt-welded to tapered steel plugs. These plugs are driven into $\frac{3}{8}$-in. holes in the web of the rails, and the rail joint is thus provided with an adequate bond. The copper strands are tin-plated, and the whole bond has the appearance of the standard galvanized-steel signal bond. This has greatly reduced the theft of bonds, which has been a source of considerable expense and annoyance where the all-copper types of bonds have been used. Bond maintenance, which used to be an item of considerable cost, has been reduced to a relatively small amount.

The continuity of return path for the traction current around the insulated rail joints required by the signal system is maintained by the use of impedance bonds. These are, in effect, composed of two auto-transformers, connected across between rails on either side of the insulated joint, and with their mid-points connected together. The impedance to the flow of traction current is therefore effectively neutralized, whereas a high impedance is maintained between rails for the signal circuits.

CATENARY CONSTRUCTION

The overhead catenary system used on main-line tracks consists of three main members—the messenger, the auxiliary, and the contact wires. On the Paoli and Chestnut Hill electrifications a $\frac{1}{2}$-in. galvanized-steel messenger wire was originally used. A few years' experience showed that this material was subject to quite rapid corrosion, particularly where steam service was present. Consequently later installations have used high-strength bronze messenger, $\frac{5}{8}$-in. diameter, with a normal tension of 4 640 lb. The specifications under which this wire is purchased permit either of two alloys—a silicon-tin-copper or an aluminium-tin-copper mixture. Both have high strength (115 000 lb. per sq. in.) and relatively low conductivity (about 13 per cent). The standard auxiliary wire is 4/0 A.W.G. grooved copper, strung to a normal tension of 1 200 lb. This member serves a dual purpose: to act as the principal current-carrying member and to act as an intermediate messenger, providing additional flexibility. The contact wire is 4/0 A.W.G. grooved bronze, either a tin-copper or a cadmium-tin-copper mixture being permitted by the specifications; the normal tension is 3 500 lb. The hanger rods between the messenger and the auxiliary wire are of bronze. The clips which are used for attaching the hanger rods to the wires and for attaching the contact wire to the auxiliary wire are of cast bronze, generally of the well known 88–10–2 mixture, with an allowable substitution of nickel for part of the tin.

The auxiliary wire is supported from the messenger at 30-ft. intervals by means of the hanger rods. These hangers vary in length from less than 3 in. near the centre of the span to about 4·5 ft. near the supports. The contact wire is supported from the auxiliary wire by means of clips, spaced at 15-ft. intervals, and so arranged that they are staggered with respect to the hangers above. This arrangement increases the flexibility of the system and makes for what is known as a "soft" wire, i.e. one without "hard spots."

The pressure of the pantograph produces an upward deflection of the overhead system which must travel as a wave ahead of the train. It is therefore easy to see that any change in grade or mass of the overhead system must be made very gradually. A pantograph moving at high speed and striking a "hard spot" has the effect of a sledge-hammer blow, both on the wire and on the pantograph. The result is that the pantograph "bounces" away from the contact wire and may cause serious damage. Consequently the greatest possible uniformity of flexibility in the catenary system is extremely important, particularly in high-speed territory.

On branch lines that are used exclusively for freight, a simple catenary is used in lieu of the compound catenary described above. In this construction the auxiliary wire is omitted and the contact wire is suspended directly from the messenger with lifting-type hangers. The loss of the high conductivity of the auxiliary wire is compensated for by using a composite messenger, having 12 copper wires on the outside and a core of either bituminous-coated galvanized steel, copper-covered steel, or high-strength bronze wires. Furthermore, the contact wire used has a conductivity of 55 per cent as compared with 40 per cent in the compound system. By this means a total conductivity practically the equivalent of that of the standard construction is obtained.

An interesting development in the details of construction has been the hollow copper rivet which has replaced bronze bolts for use with the clips. Formerly bronze carriage bolts with split ends were used, and after adjustment the ends were spread as a cotter key to hold the nut in place. There was, however, a considerable trouble with loose nuts, which frequently resulted in damage to the pantographs and wires. Approximately 5 years ago hollow rivets were adopted as standard and a machine for applying them was developed. They can now be installed as rapidly as bolts, and have proved more satisfactory.

The catenary contact system over each main running track is electrically independent of that over all other tracks, except so far as it is connected through the substation busbar. Section breaks are installed in all cross-overs and at interlockings in such a manner that the overhead system can be arranged for any movement of trains that the tracks can be arranged for. Most of these section breaks are bridged by trolley sectionalizing switches, some manually and some electrically controlled. They may be normally open or closed, depending upon the location.

EXTENT OF TRANSMISSION AND DISTRIBUTION SYSTEM

The extent of the transmission and distribution system for the electrification is indicated by the following figures: There are in service approximately 665 circuit miles of 132-kV transmission lines, 85 circuit miles of 44-kV transmission lines, and 325 circuit miles of 6 600-volt signal transmission line. There are a total of 2 278 trolley section breaks and approximately 1 500 trolley sectionalizing switches, of which 775 are hand-operated and 725 are motor-operated. The total mileage of the overhead trolley system is 1 343 track miles.

MULTIPLE-UNIT CARS

Service Requirements

Owing to geographical advantages a very extensive suburban territory has developed around Philadelphia, and the Railroad naturally desired to provide a high-class means of transportation for these communities. Also, the increase in passenger traffic had caused a seriously congested condition at the old Broad Street Station. These circumstances afforded a very attractive field for electrification, and multiple-unit cars were the only type of equipment which could economically meet the very exacting requirements. There are two outstanding points in favour of multiple-unit cars: First, their ability to accelerate and decelerate rapidly; and second, their flexibility, which permits of arranging the train size to meet the traffic demand.

The Pennsylvania Railroad had anticipated electrification and provided suburban-type cars, which were placed initially in steam service. These cars are about 54 ft. in length over body corner-posts, accommodate 72 passengers, and were built for conversion to electric multiple-unit service when desired. They are lighter in weight than the standard passenger equipment and are so arranged that the centre sill allows space for, and has sufficient strength to support, all necessary equipment beneath the car floor. The centre sill is also suitable for serving as an air duct between the ventilating fan and the traction motors. Consequently conversion to electric service required, in addition to electrical equipment, supporting brackets for equipment, application of one truck suitable for motors, and minor changes to the brake equipment. With the completion of the power-supply system to Paoli in 1915, 93 of these cars were converted to electric operation and were placed in service between that point and Philadelphia. In 1918, the line to Chestnut Hill was completed and 22 more cars were converted. Increase of traffic made it necessary to add 15 cars in 1922.

This total of 130 electrical equipments purchased in three separate lots differed in certain minor details, but in general were substantially the same. These 130 equipments are identified as having type 412-A doubly-fed motors. This type of motor represented the very latest and best in design when offered to the Railroad just before 1915. Certain changes have been made to the auxiliary apparatus of these first equipments, but the same motors, switch groups, and transformers, are still in use and giving excellent service after 20 years. It must, of course, be understood that this equipment has been repaired at times, and it is a question just how much of the original apparatus—particularly coils and windings—is still in service.

Ten years after the first cars had been placed in electric service the electrical manufacturers proposed a considerably improved control equipment along with improvements to the motor. The Railroad was already planning further electrification, and purchased 28 of these equipments for trial before deciding on the larger number which would be required. After a short period of trial 93 of the improved equipments, which are identified as having type 412-D motors, were purchased. In addition, 30 equipments very similar to the type 412-D were purchased from another manufacturer, while a third manufacturer furnished 5 equipments. These latter differed from any previously furnished in that the motors were of the series type and the control closely followed European design. This total of 128 cars was required for service from Philadelphia to Wilmington and West Chester.

With the completion of the electrification of the main line north to Trenton and the line to Norristown, another 59 cars were added in 1930. It was recognized that still more cars would be required on the completion of the electrification between New York and Washington, and 86 were obtained of a type and size which operate as motor-trailer units.

The above constitutes the single-phase multiple-unit equipment now in service. There are 431 cars operating in suburban service over 6 different lines radiating out from Philadelphia, in local service to New York and Baltimore, and in suburban service of lesser extent between New York and New Brunswick and South Amboy and between Washington and Baltimore.

Equipment

The power collected from the trolley is taken to a transformer, where the voltage is reduced from 11 000 to 850 volts for driving the traction motors. On the secondary of this transformer there are numerous taps connected to unit switches for controlling the motor voltage during acceleration. The unit switches are grouped in a box beneath the car, along with other apparatus for protection and control. Such apparatus as the blower motor, compressor, heaters, and motor-generator set, also takes power from taps of suitable voltage with the necessary control.

As the name " multiple unit " implies, any reasonable number of cars can operate in multiple as a single unit. This is accomplished in the accepted way of energizing train line wires from a master controller on the head car of the train, which control simultaneously corresponding switches on each car. This method ensures that all cars start and accelerate together. The rate of acceleration is determined and made automatic by a limit relay on each car.

The high-voltage protection is of particular interest. In view of the tremendous power resources of the trolley system, it is necessary to prevent the lowering of the pantograph while it is drawing an excessive current, such as occurs in the event of a fault. Lowering the pantograph under such conditions would cause serious flashing and burning. To prevent this, a relay is provided to operate on overload current, disconnecting all control from the pantograph to ensure its remaining against the trolley. As soon as the line is de-energized by the substation breakers the relay assumes a position calculated to lower the pantograph and hold it down until the car is taken to the shop for repairs.

Let us now consider the rating and characteristics of this equipment. Each of the single-unit cars carries two motors rated separately at approximately 200 h.p., which are sufficient to accelerate the car at 1 mile per hour per sec. and drive it continuously at 45–65 m.p.h. The double-unit cars also have but two motors, both mounted in one truck, rated at 370 h.p. each. Although differing in almost every detail of design, these double-unit cars are arranged to multiple with the older single units.

There are several features of the multiple-unit equipment, regardless of make and design, which are of interest. Automatic protection is provided for motor failures, slipping of wheels, and loss of ventilating air. Electro-pneumatic braking with clasp brakes on all wheels is used, so that very rapid yet smooth deceleration is possible. This feature is as essential as rapid acceleration in order to maintain the fast schedules now provided.

Along with the progress by electrical manufacturers in propulsion and control equipment, the Railroad has improved numerous features to provide better service and reduce the cost of maintenance. Probably the first change of consequence was that of the body lighting. Originally, the power for this purpose was taken from the transformer and was subject to all the fluctuations in line voltage; also, it was not satisfactory, coming from a 25-cycle source. Direct current is now used for lighting, taken from a small motor-generator set and battery. Following this, the headlights were increased to 250 watts, equivalent to those on all locomotives. This was a very desirable improvement from a safety standpoint. To provide more dependable air supply for braking and operation of certain pneumatic equipment, the original compressors have been eliminated and a much improved 4-cylinder design adopted. Thermostatically-controlled heat is now provided, with a rearrangement which increases the amount of heat available for the car. Another feature making for improved operation has been the adoption of the code-type 4-indication cab-signal system with whistle and acknowledger. A continuous signal indication is carried in the motor-man's compartment. This permits running on closer schedules and is of great assistance to the motorman during foggy weather.

Maintenance

The Railroad is fully equipped to maintain and repair all its car equipment, even to the extent of manufacturing many of the parts required. Transformers, motors, and pantographs are rebuilt, relays rewound, and control overhauling done, in the Railroad's shops. The heavy repairs and maintenance work are done at Wilmington and the periodic inspection at Paoli. Originally, this periodic inspection was on a 1 500-mile basis; later it was increased to 3 000 miles, then to 4 500 miles, and now after inspection a car does not return to the shop for periodic attention until it has covered 6 000 miles. This system necessitates very careful inspection and supervision, but it has materially reduced the maintenance cost and permits the inspection of 431 cars with the same facilities as were originally provided for maintaining approximately 100 cars.

ELECTRIC-LOCOMOTIVE DEVELOPMENTS

The electric locomotives used by the Pennsylvania Railroad have been the result of an orderly and consistent development as permitted by the development of motors and other electrical parts. This development has progressed continuously from about 1905 up to the present day.

The first two electric locomotives constructed by the Pennsylvania Railroad were built as an experiment in 1905. They were of very similar design, each having two swivel trucks, articulated together and surmounted by a box cab. One of these locomotives had geared motors and the other had gearless motors. Both locomotives are still in, or available for, regular service. After construction they were tested on a special test track installed on the West Jersey and Seashore Railroad for the purpose of measuring accurately their action on the track. Like all locomotives of this design they were very rough at the higher speeds.

In 1908 the construction of the new terminal station and tunnels in New York was well under way and a decision on the power system and the selection of suitable locomotives had to be made. The New Haven Railroad was successfully operating with single-phase alternating current, and the Pennsylvania Railroad at this time was cognizant of the probable advantages of that system for heavy main-line service. In the autumn of 1908, five miles of track on the central extension of the Long Island Railroad, east of Garden City, were equipped with overhead catenary-wire construction, and tests were made of a 15-cycle gearless locomotive and of a car equipped with 15-cycle motors. The overhead wire comprised sections of all the types of construction that gave any promise. The locomotive and car, as well as several other trailer cars, were equipped with various types of current collectors, pantograph shoes, etc. The locomotive and car were operated in test service day and night for several months, and considerable information was thus obtained as to the tracking action of the locomotive and as to the performance of the various types of wire and of current collectors. After consideration of the results it was decided to use direct current with a third rail between Manhattan Transfer and Sunnyside Yard, including the river tunnels and station area.

In 1909, two experimental direct-current locomotives known as the DD-1 type were built and tested on the Long Island Railroad. These locomotives consisted of two articulated units operating as a single engine. Each had a four-wheel truck and two driving axles. The motors were located in the cabs, to give a high centre of gravity with easy action on the track, and the drive was transmitted to the wheels by rods and a jack shaft. These test locomotives were operated day and night during the winter of 1909 and 1910, hauling a train loaded with scrap iron. Their operation was very satisfactory and, after a few minor modifications had been made, 24 more locomotives were constructed early in 1910. During the summer of 1910, prior to the opening of the service into the New York terminal station, the test track mentioned previously was installed on the line west of Hackensack Portal and the engines were tested for track action. A service of through trains out of the New York terminal was started in November, 1910. Seven more locomotives of the same type were subsequently built to handle the increasing service. All of these type DD–1 locomotives are still in, or available for, service. They were the last ones constructed by the Pennsylvania Railroad for purely d.c. operation. All locomotives built thereafter were so designed that even though they operated on direct current they could be readily converted to alternating current by minor changes. This was in line with the established policy of using 25-cycle alternating current for future extensions of electrification.

Single-Phase Locomotives

In 1917, foreseeing the possibility of main-line electrification for through trains, the Railroad built a 4 000-h.p. freight locomotive having 3-phase induction motors and the necessary phase convertor to permit operation on the single-phase trolley wire. This locomotive had speeds of 10 and 20 m.p.h. and was especially adapted to heavy-grade work, since it automatically regenerated. This locomotive was placed in freight service on the section electrified for suburban service between Philadelphia and Paoli, and was the most powerful one of its type constructed up to that time.

About 1922 it became apparent that main-line electrification was imminent. Experience and study had indicated that the Railroad needed a more flexible locomotive than could be provided with induction motors. Four tracks were available on the main line, with trains of various classes operated at varying speeds. Hence it was apparent that a commutator-type motor would be necessary to give the flexibility desired. The Railroad further conceived the idea of a locomotive that could be used either as a high-speed passenger locomotive or as a lower-speed freight locomotive with high drawbar pull, by changing the gear ratio. At this time motors which could be placed between a pair of driving wheels were not available of sufficient power to utilize properly the weight on the wheels, and the new locomotives were designed with the motors located near the ends of the locomotives beyond the driving wheels and connected thereto by jack shafts and side rods. These locomotives were known as the L–5 class. They had four pairs of driving wheels in a rigid frame, with a two-wheeled truck at either end. In 1924, three locomotives were built, one for operation on alternating and the remainder on direct current. These were followed shortly by 21 more d.c. locomotives.

In the next few years radical improvements were made in the design of motors, which resulted in twin motors of 1 250 h.p. that could be placed between a pair of driving wheels above the axle, and thereby permit the elimination of jack shafts and side rods. The Railroad constructed experimental engines of three types using these new motors: (1) A 2 500-h.p. passenger locomotive for light passenger service, known as the O1 class; (2) a 3 750-h.p. passenger locomotive for heavy service, known as the P5 class; (3) a 2 500-h.p. freight locomotive using four axle-hung single motors, known as the L6 class. The electrical equipments of these three classes of locomotives were identical except for the size of transformer and the type of motor frame. The mechanical parts were also made as nearly interchangeable as possible. The two sizes of passenger locomotives could be coupled together, if necessary, to provide power units of 2 500 h.p., 3 750 h.p., 5 000 h.p., 6 250 h.p., etc. The freight engines provided power units of 2 500 h.p., 5 000 h.p., and 7 500 h.p., etc. After extensive road tests of these experimental locomotives, 62 more of the large passenger type (P5 class) were built for the service between New York and Philadelphia.

Final Locomotive Types

During the first year of electric operation between New York, Wilmington, Philadelphia, and Paoli, the electric locomotives handling this service covered 4 647 934 locomotive-miles, and their operation was subjected to a very complete study by the Railroad Company, not only as to performance but also in order to obtain additional information for the analysis of electrically-operated passenger and freight schedules.

The original programme contemplated building 28 additional passenger locomotives for the extension of the passenger service from Wilmington to Washington, and parts of these locomotives had already been constructed and stored at the works of the manufacturers. Sixty locomotives for freight service only had been contemplated, and two of these had been built and placed in service during 1933. Experience indicated that better freight operation could be secured by the use of more powerful and higher-speed locomotives than the two experimental freight locomotives; and also that the requirements of the through passenger service were such as to make desirable the use of more powerful and faster passenger locomotives than those in use between New York and Wilmington.

It was therefore decided to transfer to the freight service 58 of the passenger locomotives already operating between New York and Wilmington, and to substitute 58 locomotives of somewhat different design, greater capacity, and higher speed. By so doing there would be secured in both the passenger and the freight service the benefit of higher-speed and more powerful locomotives than were provided in the original programme. There would also be the advantage of having in freight service a large number of locomotives which would be available also for high-speed passenger service.

It was also decided that, in future locomotives, the motorman should be located in the middle rather than at the end of the cab, as this arrangement facilitated desirable streamlining. Starting early in 1934, two new types of passenger locomotives of higher speed and greater horse-power than the existing locomotives were designed. At the same time the existing type P5 locomotive, 28 of which had been partly completed, was redesigned. By September of the same year one experimental engine of each of the two new types was on the rails ready for a test. Rigid comparisons were made to determine their relative advantages as to performance on the track, cost, maintenance, efficiency, etc., and before the end of the same year a decision had been reached.

The two new types were known as the class R1 and the class GG1, the former having four driving axles in a rigid wheelbase with a 4-wheel truck at each end, and the latter having two main frames articulated together, each main frame comprising three driving axles and a 4-wheel truck. The cab framing of the latter locomotive was constructed in the form of a truss to give longitudinal strength, and the cab rested on the main frames on centre plates. The GG1 locomotive was the one selected to be built in quantity. It is rated at 4 620 h.p. (continuous) with a maximum short-time rating of about 9 000 h.p. It is built for 90 m.p.h., but tests show that it may be safely operated at speeds well in excess of 100 m.p.h.

Track Tests

The comparative track tests of the various types of locomotive are of special interest. The test track consisted of 440 ft. of special track having steel ties on which the rail was free to move laterally except as restrained by recording devices located at both ends of every alternate tie. The recording device was of the Brinell type. Lateral pressure of the wheel flanges as the locomotive passed along the rail tended to push the rail outward and this, in turn, pressed a hard steel ball against a soft steel plate. The size of the indentation gave a measure of the flange pressure against the rail. The later types of engine also had applied to them other testing devices which recorded the lateral pressure at the wheel hubs and gave a continuous record of wheel pressures against the rails, and still other devices which recorded movement, acceleration, etc., at various points. Engines so equipped were operated between Wilmington and New York at speeds up to 90 m.p.h. or more, so as to get all available data before the final selection was made of the type to be built.

Extent of Locomotive Development

The figures in the Table give some idea of the development as regards weight, horse-power, etc., in the Pennsylvania Railroad's electric passenger locomotives over the past 30 years.

Table

Class	Total weight	Motors			Length	Locomotive weight
	tons	lb.	h.p.	lb./h.p.	ft. in.	lb./h.p.
DD1 (D.C. Passenger)	313 100	89 200	1 580	56·4	64 11	198
L5 (D.C. Passenger)	395 400	84 000	3 730	22·5	68 2½	106
L5 (A.C. Freight)	408 600	84 000	3 070	27·3	68 2½	133
O1C (A.C. Passenger)	300 000	59 200	2 500	23·6	52 8	120
P5A (A.C. Passenger and Freight) ..	394 000	88 800	3 750	23·6	62 8	105
L6 (A.C. Freight)	300 000	64 000	2 500	25·6	51 10	120
R1 (A.C. Passenger)	402 000	68 920	5 000	13·75	64 8	81
GG1 (A.C. Passenger)	460 000	79 338	4 620	17·1	79 6	100

INAUGURATION OF ELECTRIC SERVICE

When the final arrangements had been made in December, 1933, for financing the continuation of the electrification programme, immediate steps were taken to complete the 28 electric passenger locomotives of the older design already partially constructed at the works of the manufacturers, and to design and construct the 58 new passenger locomotives of higher speed and greater power. With the engineering advice and assistance of the General Electric Co., the Westinghouse Co., and the Baldwin Locomotive Works, this work was pushed to rapid completion. The last of the 57 new GG1 engines was received in August, 1935, less than 18 months after the design work had been started.

With the co-operation of the public officers in the cities affected, the roadway construction was also finished on schedule, and the first electric test train was run from Wilmington to Washington on the 20th January, 1935; regular passenger train service was inaugurated on the 10th February. The freight electrification was completed on schedule, and service was begun in May and built up as fast as new locomotives were received.

OPERATING AND MAINTENANCE ORGANIZATION

Generally the operation and maintenance of the electrification roadway facilities and motive-power equipment have been assimilated into the departments handling similar work in steam service, in such a way as to keep the costs at a minimum. The motive power is operated by crews trained from the roster of former steam service employees, and maintenance of the motive power is handled in each division by the forces under the master mechanic. The roadway facilities are maintained in each division by the forces under the division engineer and supervisor of signals. In the operation of the substation and trolley switching and power supply, however, it has been necessary to set up an organization on similar lines to the train-dispatching organization. Each division or zone of the electrification has a power director, who is responsible for all switching moves in his territory. The actual operation of the switches is performed remotely from the adjacent signal tower by the signalman except where supervisory control has proved more economical, in which cases control is performed directly by the power director. The power directors are responsible to the supervisor of signals and are furnished with complete telephone service to all points of control, to the train dispatchers, and to all telephone locations along the right-of-way. Each power director is equipped with a " model board " on which the circuits in his area are shown in miniature with manually-operated red and green lights to show the position of all switching, enabling him to know at all times the condition of the circuits and thus to act promptly and intelligently.

In addition it has been found essential to have separate centralized supervision over the transmission circuits and power supplies, so that switching and manipulation of circuits locally will not interfere with inter-divisional circuits, and so that the supply of power will be controlled economically and effectively over the entire electrification. The responsibility for this rests with a system load-dispatcher, who is located at Philadelphia and reports to the electrical engineer. He is equipped, in a manner similar to the power director, with a " model board." For his requirements the board indicates all power-supply stations, transmission circuits, and substation connections, over the entire electrified area. Trolley switching and local circuits are omitted from his board. He is connected by a telephone system to all power directors and power-supply stations, and has an elaborate telemetering system to enable him to follow load conditions and division of load among the various supply stations.

TRAFFIC

Over the present single-phase electrification operated by the Pennsylvania Railroad, covering a route of 373 miles and having 1 343 miles of electrified track, are electrically operated daily about 50 freight trains and over 650 passenger trains, including 450 multiple-unit trains, in and out of the terminals at New York, Philadelphia, Baltimore, and Washington. During the year ending the 30th June, 1936, this service has produced 12 145 293 multiple-unit car-miles and 83 330 290 passenger car-miles, with an electric locomotive mileage of 8 806 376. In freight service over the same period 2 820 565 000 gross ton-miles were hauled, requiring 2 046 687 electric freight locomotive-miles. Over 1 000 000 electric locomotive-miles were required for switching and other service. The maximum monthly mileage made during the year by an electric locomotive was 13 596, and the total mileage made by the 58 type GG1 high-speed passenger locomotives, after averaging about 1 year in service, was 6 968 581. To handle the a.c. electrified service the electrical energy required for the year amounted to 526 870 545 kWh. The maximum power required during 1 hour was 142 700 kW.

CONCLUSION

Early in its development electrification showed conclusively that for handling heavy suburban passenger service in and out of terminals in densely-populated cities it was the outstanding means of motive power, and as experience and familiarity with its use developed it became evident that it had extensive possibilities for handling heavy passenger and freight service over long distances where sufficient traffic existed to justify the generation of power in large quantities in central locations and its distribution electrically to moving trains.

Over the Pennsylvania Railroad between New York and Washington moves some of the densest rail traffic in the world, and this is therefore one of the portions of the Railroad best suited to the application of electric operation. The electrification is now producing substantial economies over the steam operation which it replaced, and is giving the public a faster, better, and more reliable service.

R.M.A. SPECIFICATION FOR TESTING AND EXPRESSING THE OVERALL PERFORMANCE OF RADIO RECEIVERS*

(*Specification prepared by the* RADIO MANUFACTURERS' ASSOCIATION, *and discussed before the* I.E.E. WIRELESS SECTION *3rd February*, 1937.)

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PART I. ELECTRICAL TESTS

SECTION 1.—GENERAL

(1) Scope and Application

1.1 This Specification applies to all radio receivers, or the radio portion of radio-gramophones, except where variable reaction is used.

1.2 The wave bands considered are from 150 kilocycles per sec. to 30 megacycles per sec.

1.3 The Specification is framed primarily for open-aerial receivers, but frame-aerial receivers may be included if the method of input coupling described in the following paragraphs 4.1 to 4.3 be substituted wherever coupling by dummy aerial is specified.

(2) Definitions

2.1 Standard Output:

The standard output is 50 milliwatts. This shall be measured in a non-inductive load connected across the output transformer in place of the voice coil or operating winding. The resistance of this load shall equal the modulus of the voice-coil impedance measured at 400 cycles per sec. Where outputs other than the standard output are quoted they shall be measured in the same way.

2.2 Sensitivity:

The sensitivity of a radio receiver, intended for use with an open aerial, shall be expressed as the input in microvolts (absolute) needed to give the standard output. The input referred to is the open-circuit voltage generated by the signal generator.

Should it be inconvenient to measure a particular receiver at the standard output, this output may be increased and the result reduced to conform with the definition given above.

2.3 The sensitivity of a frame-aerial receiver shall be expressed as the input in microvolts per metre needed to give the standard output.

Note.—It must be noted that the sensitivity of a frame-aerial receiver and of an open-aerial receiver as defined above are not strictly comparable as measures of performance.

2.4 Selectivity:

The selectivity of a radio receiver is expressed as the ratio of an interfering carrier to an adjacent wanted carrier to which the set is tuned. The wanted carrier is unmodulated and of specified strength, and the interfering signal is modulated and of such a strength as to give a specified output at the frequency of this modulation.

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* This Specification has been drafted by the Technical Advisory Committee of the Radio Manufacturers' Association. Its aim is to assist manufacturers by suggesting standardized methods of measuring and expressing the overall performance of radio receivers. Certain routine tests, such as tests for electrical fidelity, low-frequency gain, or electrical output, though they may be useful in design work, are not included because they are not simply related to overall performance.

2.5 Noise:

The noise of a receiver comprises all frequencies present in the output other than the wanted signal and mains hum. Noise due to external disturbances is excluded.

2.6 Automatic Volume Control:

This refers to a carrier-operated device which reduces the sensitivity of a receiver as the input increases. The performance is expressed as a relation between the change of output and the corresponding change of input.

(3) Apparatus Required

3.1 Signal Generator:

Signal generators shall give readings of microvolts to ± 10 % up to 10 megacycles per sec. and to ± 25 % above 10 megacycles. Their frequency calibration shall be within 1 % or within 10 kc. whichever is the greater, and incremental frequencies shall be within 0·5 kc.; the modulation shall be accurate to within 1/10 of the nominal percentage.

3.2 Dummy Aerial:

For medium- and long-wave measurements up to 2 000 kc. the dummy aerial shall comprise three series elements, viz. a resistance of 25 ohms, an inductance of 20 μH, and a capacitance of 200 $\mu\mu$F. These elements shall include the internal impedance of the generator and all connections.

For short-wave measurements above 2 megacycles per sec., the dummy aerial shall consist of a 400-ohm series resistance, which shall include the impedance of the generator and all connections.

3.3 Shielded Coil:

This shall be a cylindrical coil, 5 cm. in radius and 6 cm. deep, wound with 20 turns to an approximate inductance of 40 μH. The whole coil shall be shielded by a wire cage arranged to avoid magnetic screening (i.e. there shall be no completed circuits whose planes are normal to the axis of the coil). The connecting leads shall also be screened.

(4) Method of Coupling to Frame-Aerial Receivers

4.1 The signal generator shall be connected to the shielded coil specified in 3.3.

4.2 Either the inductive reactance of the coil shall be so large compared with the 1 000 kc. internal impedance of the source (say 20 times) that open-circuit conditions may be assumed to hold, or a suitable load correction shall be applied.

The coil and frame-aerial shall be co-axial. The distance between the geometric centres shall be 2 metres for sensitivity measurements, and shall be varied for selectivity measurements as detailed in Section 3, par. 1.3.

4.3 The field in microvolts per metre at the frame aerial shall be taken as

$$\frac{18\,800 N r^2 V}{(r^2 + D^2)^{3/2} \omega L}$$

where

V = microvolts indicated by the signal generator.
N = number of turns on the coil.
r = radius of the coil in cm.
D = axial distance in cm. between the geometric centres of the coil and the frame aerial.
L = inductance of the coil in henrys.

This equation reduces for the coil and distance specified to

$$\frac{1{\cdot}174V}{\omega L} = 4\,670V/f$$ approximately, where f is the frequency

(5) Standard Precautions

5.1 Before making measurements, the receiver shall be fitted in its cabinet, under the conditions in which it will normally be used, and switched on for at least half an hour.

5.2 All batteries shall show their nominal operating voltages. The internal resistance of the H.T. battery shall be increased by adding, in the negative H.T. lead, a resistance equal to 1 ohm per cell. Care shall be taken to see that the added resistance does not affect the grid bias.

5.3 Mains-operated receivers shall be operated at such voltages as the user would be expected to apply ideally. In general, where the receiver carries a mains tapping device and a number of marked ranges, the applied voltage should fall in the middle of one of these ranges, the midpoint being calculated as a geometric mean.

5.4 No adjustments, other than those available to the ordinary user, shall be made to a radio receiver during the course of these performance tests.

SECTION 2.—SENSITIVITY

(1) Method of Measurement

1.1 Measurements shall be made through the appropriate dummy aerial as specified in Section 1.

1.2 Measurements shall be made at the following frequencies, so far as they apply to the particular receiver:—

Long-wave band:—160 kc.; 200 kc.; 300 kc.
Medium-wave band:—600 kc.; 1 000 kc.; 1 400 kc.
Short-wave band:—At one frequency in the middle of each of the accepted short-wave bands, which are at present as follows:—

26·6 —25·6 megacycles per sec.
21·55—21·45 megacycles per sec.
17·8 —17·75 megacycles per sec.
15·35—15·11 megacycles per sec.
11·9 —11·7 megacycles per sec.
9·6 — 9·5 megacycles per sec.
6·15— 6·0 megacycles per sec.

The above frequencies shall be modulated at 400 cycles per sec. to a depth of 30 %.

(2) Method of Expressing Results

2.1 The sensitivity, as defined in Section 1, par. 2.2, may be plotted as a smooth curve against frequency, or may be given in tabular form at the discrete frequencies specified in par. 1.2.

2.2 Curves shall be plotted on the following paper:—

Sensitivity ..	Vertical scale, 4-cycle logarithmic.
Frequency ..	Horizontal scale, Long waves: 2 mm. to 10 kc. Medium waves: 1 mm. to 10 kc. Short waves: Scale to be chosen to suit manufacturers' requirements. Heavy vertical lines shall be intercalated every centimetre.

SECTION 3.—SELECTIVITY

(1) Method of Measurement

1.1 This measurement applies at present only to long- and medium-wave bands, i.e. between 150 kc. and 2 000 kc. Two similar signal generators shall be connected in series and applied to the receiver through the medium- and long-wave dummy aerial. Care shall be exercised to ensure that each signal generator functions correctly, independently of the series connection, and that the requirements specified in Section 1 for the total impedance of the dummy aerial are met.

1.2 When measuring the selectivity of an open-aerial receiver, the receiver shall first be tuned to the signal generator which is to provide the interfering signal. This signal shall be modulated at 400 cycles per sec. to a depth of 30 %, and set to a strength of 1·0 mV. The volume control shall then be set so that the receiver delivers one-quarter of its nominal power output. The second signal generator, giving the unmodulated wanted signal, shall then be switched on and its frequency adjusted to that of the first generator by the method of zero beat. (For this purpose the modulation may be removed from the first generator.) The strength of the second signal shall be set to 1·0 mV.

The first (interfering) signal generator shall then be detuned by various positive and negative increments, beginning at 5 kc., and the strength at each incremental measurement so adjusted that the output meter indicates an output of interference 40 db. below the output power first determined, i.e. below one-quarter of the nominal output of the receiver.

A filter whose characteristics are such as effectively to remove all frequencies other than the 400-cycle signal, shall be included between the receiver and the output meter.

1.3 For frame-aerial receivers, the method described in 1.2 above shall be modified as follows: The two signal generators in series shall be coupled to the receiver by means of the shielded coil and in the manner specified in Section 1, par. 4. The distance between the geometric centres of the coil and the aerial should be 2 m. Particular attention is called to the provision in Section 1, par. 4.2, for applying a correction to the generator readings when the total internal impedance of the two generators in series is appreciable compared with the 1 000-kc. reactance of the coupling coil.

The receiver shall first be tuned to the signal generator which is to provide the interfering signal. This signal shall be modulated at 400 cycles per sec. to a depth of 30 % and set to produce a field of 1 mV per metre at the frame aerial in accordance with the formula in Section 1, par. 4.3. The volume control shall then be so set that the receiver delivers one-quarter of its nominal power output to the load. The signal-generator output shall then be changed to 1 mV and the distance between the receiver and the coupling coil reduced until the output meter shows that the receiver is again delivering exactly the same power, the receiver controls being unaltered.

The second signal generator, giving the unmodulated wanted signal, shall then be switched on and its frequency adjusted to that of the first generator by the method of zero beat. The strength of this signal shall be set to 1 mV.

The first signal generator shall then be detuned, following the procedure of par. 1.2.

(2) Method of Expressing Results

2.1 The wanted signal having a strength of 1 mV, the selectivity is expressed as the strength in millivolts of the unwanted carrier. The selectivity may be expressed for increments of 9 kc. from each of the frequencies specified in 1.2. If the selectivity is different for positive and negative increments, the mean shall be given.

2.2 The selectivity may also be expressed as a family of smooth curves, one for each of the frequencies of 1.2. The following paper shall be used:—

Selectivity ..	Vertical scale, 4-cycle logarithmic.
Frequency (Incremental) ..	Horizontal scale, 2 mm. to 1 kc. Heavy vertical lines intercalated at every 5 kc.

2.3 The output power at which the measurement was made shall be stated.

SECTION 4.—NOISE

(1) Method of Measurement

1.1 The noise level of a receiver is defined for the purpose of this Specification as the radio-frequency input carrier (unmodulated) for which the r.m.s. noise output from the receiver is equal to the output given by a 10 % modulation of that carrier at 1 500 cycles per sec.

1.2 The noise level shall be measured at all frequencies at which sensitivity is measured.

1.3 A signal at one or other of the above frequencies shall be applied through the standard dummy aerial. This signal shall be modulated at 1 500 cycles per sec. to a depth of 10 %, and shall be adjusted to such a strength that, when the modulation is switched off, the output in watts registered by the output meter is halved. This input, expressed in microvolts, shall be termed the noise level of the receiver. Accurate tuning is essential.

1.4 The output meter shall include a filter which shall effectively remove frequencies of 100 cycles per sec. and lower, and the combination shall have a characteristic which is flat within 2 db. from 500 to 8 000 cycles per sec.

1.5 Where a sensitivity control is provided, it shall be set for maximum sensitivity. The position of the tone and selectivity controls, where provided, shall be stated.

1.6 It is essential to exclude noises picked up from the mains or other external sources.

(2) Method of Expressing Results

2.1 The method of expressing the results is included in the definition of 1.1, par. 1, above.

SECTION 5.—AUTOMATIC VOLUME CONTROL (A.V.C.)

(1) Method of Measurement

1.1 A.V.C. performance shall be measured at one representative frequency in each of the wave bands provided by the receiver. The signal shall be modulated at 400 cycles per sec. to a depth of 30 %, and shall be applied through the appropriate dummy aerial as specified in Section 1.

1.2 The input shall be adjusted to a value of 1 volt and the volume control set so that the receiver delivers one-quarter of its nominal power output.

The input shall then be reduced in suitable steps to zero, and the output read on the output meter at each step.

(2) Method of Expressing Results

2.1 The relation between input and output may be plotted as a smooth curve.

2.2 The following paper shall be used:—

Output, in watts..	Vertical scale, 4-cycle logarithmic, with an auxiliary decibel scale.
Input, in microvolts	Horizontal scale, 6-cycle logarithmic, with an auxiliary decibel scale.

2.3 Where it is desired to condense the information supplied by A.V.C. curves, the mean slope of the curve may be quoted. In estimating the average slope, the manufacturer shall state the input-voltage limits between which the average has been taken. The slope shall be stated as the change in input level, expressed in db., needed to produce a change of 1 db. in the output.

PART II. ACOUSTIC TESTS

SECTION 1.—GENERAL

(1) Scope and Application

1.1 This Specification applies to all radio receivers and electric gramophones, except that receivers using variable reaction are excluded.

(2) Definitions

2.01 Frequency Response:

The frequency response of a receiver or gramophone is a relation between the intensity level at a specified point and the frequency of the audio signal.

2.02 Intensity Level:

The intensity level of a signal is its ratio, expressed in decibels, to a reference power flow of 10^{-16} watt per cm^2. This power may be taken as equivalent to a pressure of 0·000204 dyne per cm^2.

2.03 Loudness Level:

The loudness level of a signal is the intensity level of a 1 000-cycles-per-sec. note which an average listener judges to be equally as loud as the signal. Loudness level is expressed in phons.

The following Table shall be taken to represent the relation between intensity level and the loudness level of a 400-cycles-per-sec. pure tone:—

Intensity Level, db.	*Loudness Level, phons.*
10	0
20	11
30	22·5
40	35
50	47
60	58
70	69·5
80	80
90	91
100	99

2.04 Overall Acoustic Sensitivity:

The overall acoustic sensitivity of an open aerial receiver is the input in microvolts needed to give a loudness level, at a defined point, of 50 phons, the carrier being modulated at 400 cycles per sec. to a depth of 30 %.

The overall acoustic sensitivity of a frame-aerial receiver is the input in microvolts per metre needed to give the above output, measured under similar conditions.

2.05 Distortion Factor:

For the purposes of this Specification, distortion factor is defined as

$$\frac{1}{2}\sqrt{\left[\frac{\sum_{n=2}^{n=\infty} n^2 V_n^2}{\sum_{n=1}^{n=\infty} V_n^2}\right]}$$

where V_n is the voltage across the load, representing the voice coil or operating winding, due to the nth harmonic ($n = 1$ for the fundamental).

2.06 Total Harmonic Content:

The total harmonic content is defined as

$$100\sqrt{\left[\frac{\sum_{n=2}^{n=\infty} V_n^2}{\sum_{n=1}^{n=\infty} V_n^2}\right]}$$

where V_n is the voltage due to the nth harmonic.

2.07 Electrical Output:

Where electrical output is referred to it is to be measured in a pure resistance load equal to the modulus of the 400-cycles-per-sec. impedance of the voice coil or operating winding.

2.08 Free Space Conditions:

Free space conditions imply:—

(*a*) Still air.
(*b*) Absence, at all frequencies, of reflected waves.
(*c*) No distortion of the sound field by the microphone.

2.09 Commercial Approximation to Free Space Conditions:

A commercial approximation to free space conditions may be obtained out of doors at a considerable distance from the ground or other reflecting surfaces, or in a heavily damped room.

The approximation shall be considered satisfactory if the results obtained in a damped room do not differ by more than 3 db. at any point from those obtained in free space.

The minimum internal volume of the room, after treatment, should be 1 000 cu. ft.

2.10 Hum:

By hum is meant low-frequency noises due to such causes as inadequate filtering of rectified voltages, mechanical vibration of mains transformers, etc. Only hum traceable to a.c. mains is here considered. Hum is expressed as a loudness level in phons.

(3) Apparatus Required

3.1 Signal Generator:

The signal generator used shall give readings of microvolts accurate to 20 %. The frequency calibration shall be accurate within 20 kc. at 1 000 kc. The percentage modulation shall be accurate to within 1/10th of the nominal percentage. The total harmonic content of the modulation envelope, for the purpose of acoustic sensitivity and frequency response measurements, shall not exceed 5 %. For measurement of acoustic output, the distortion factor of the modulated envelope should be as small as possible; otherwise the range of acoustic output over which the overall distortion factor can be measured may be seriously limited.

3.2 Audio-Frequency Oscillator:

The audio-frequency oscillator shall be capable of giving, in conjunction with the signal generator, the requirements stated in par. 3.1. A filter may be used to remove the harmonics of 400 cycles per sec.

3.3 Microphone:

The microphone shall be pressure-operated and shall be calibrated in volts per dyne per cm^2 to an accuracy of 10 %. A field calibration is required.

The voltage output may conveniently be the open-circuit output, a suitable correction being applied if the microphone is connected to a finite load.

3.4 Dummy Aerial:

This shall be the medium- and long-wave dummy aerial specified in Part 1 of this Specification.

3.5 Shielded Coil (for Frame-Aerial Receivers):

This coil shall be as specified in Part 1 of this Specification.

(4) Standard Precautions

4.1 Before making measurements, the receiver shall be fitted in its cabinet under the conditions in which it will normally be used and shall be switched on for at least half an hour.

4.2 All batteries shall show their nominal operating voltages. The internal resistance of the H.T. battery shall be increased by adding, in the negative H.T. lead, a resistance equal to 1 ohm per cell.

Care shall be taken to see that the added resistance does not affect the grid bias.

4.3 Mains-operated receivers shall be operated at such voltages as the user would be expected to apply ideally. In general, where the receiver carries a mains tapping device and a number of marked ranges, the applied voltage should fall in the middle of one of these ranges, the midpoint being calculated as a geometric mean.

4.4 No adjustments, other than those available to the ordinary user, shall be made to a radio receiver during the course of these performance tests.

SECTION 2.—FREQUENCY RESPONSE

(1) Conditions of Measurement

1.1 Measurements shall be made in free space conditions or in the commercial approximation to free space conditions defined in Section 1.

1.2 The frequency response of a gramophone shall be measured with calibrated records.

1.3 The frequency response of an open-aerial radio receiver shall be measured with an input of 20 mV at 1 000 kc. modulated to a depth of 30 %. The input shall be applied through a standard dummy aerial. The tone control and variable selectivity control, where provided, shall be set for maximum high-note response unless otherwise stated.

1.4 The frequency response of a frame-aerial receiver shall be measured with an input of 20 mV per metre obtained from a shielded coil in the manner specified in Section 1, par. 4, of Part 1 of this Specification. The tone control and variable selectivity control, where provided, shall be set for maximum high-note response unless otherwise stated.

1.5 The power delivered to the voice-coil or operating winding shall be set by means of the volume control to one-quarter of the nominal power output of the receiver at 400 cycles per sec.

1.6 Where a receiver is fitted with variable noise suppression, the control shall be set at either its maximum or its minimum position.

(2) Method of Measurement

2.1 The essential elements of the measuring apparatus are:—

(*a*) A low-frequency oscillator as defined in Section 1, par. 3.2.

(*b*) A microphone as defined in Section 1, par. 3.3.

(*c*) (For radio receivers.) A signal generator as defined in Section 1, par. 3.1.

(*d*) A microphone amplifier and output meter. Results shall be corrected to within ½ db. for the frequency characteristic of this device and of the microphone.

(*e*) (For noisy locations.) A filtering device to eliminate extraneous noises or electrical disturbances.

2.2 Hand-operated, automatic, or semi-automatic recording may be used, provided that all maxima and minima are recorded. Stopping the apparatus at any one frequency shall not change the registered intensity level by more than 2 db.

2.3 Two series of measurements shall be made, viz.:—

(*a*) On the axis of the speaker, at a distance of 3 ft., measured between the plane of the microphone diaphragm and the plane of the speaker grille or final opening of the cabinet. The plane of the microphone diaphragm shall be perpendicular to this axis.

(*b*) At a point 45° from the axis, measured in a horizontal plane, at the same distance, defined in a similar manner. The plane of the microphone diaphragm shall be perpendicular to the line joining microphone and speaker.

Where any difficulty is found in locating the microphone in accordance with the above definition, the position adopted shall be clearly described.

Where the microphone used has no diaphragm, this clause shall be interpreted by reference to the conditions obtaining during the calibration of the microphone.

The centre of the speaker shall be at least 3 ft. from any boundary.

(3) Method of Expressing Results

3.1 Two curves shall be plotted on the same sheet, viz. the axial response and the 45° response.

3.2 The curves shall be plotted on 3-cycle mm. paper. The abscissae shall cover a range of 30 to 10 000 cycles per sec., but the portion of this available range to be plotted for publication is at the discretion of the manufacturer. The ordinate scale shall be 2 mm. per db.

The geometric mean of the measured pressure between 200 and 600 cycles per sec. (obtainable directly from the curve) shall be converted to an intensity level by referring it to a pressure of $0 \cdot 000204$ dyne per cm^2. This level shall be taken as the 400-cycles-per-sec. intensity level and shall be used as the zero line of the curves.

3.3 Published curves shall bear the following information:—

(*a*) The position (max. or min.) of the variable noise-suppression control where fitted.

(*b*) Where gramophone needles are used, the type shall be stated.

SECTION 3.—ACOUSTIC SENSITIVITY

(1) Conditions and Method of Measurement

1.1 The acoustic sensitivity is calculated from the frequency-response measurements described in Section 2, and the electrical sensitivity measurements described in Section 2, Part 1, of this Specification. Reference should be made to these Sections for the conditions and method of measurement.

(2) Method of Expressing Results

2.1 The acoustic sensitivity shall be stated for each frequency quoted in Part 1, Section 2, par. 1.2.

2.2 The acoustic sensitivity shall be calculated from the following formula:—

$$V_A = V_E \sqrt{\left[\frac{W}{0 \cdot 05 \times 10^{(I_W - I_O)/10}}\right]}$$

where V_A = acoustic sensitivity (mV).
V_E = electrical sensitivity (mV).
W = 400-cycle power in the load (approx. one-quarter of the nominal output).
I_W = 400-cycle intensity level as defined in Section 2, par. 3.2.
I_O = 400-cycle intensity level of 53 db., corresponding to an output of 50 phons.

SECTION 4.—HUM

(1) Conditions of Measurement

1.1 Hum measurements shall be made under the commercial approximation to free space conditions specified in Section 1.

1.2 Electrical pick-up, and extraneous noises due to microphone amplifier, room disturbances, etc., shall be at least 5 phons below the lowest hum level to be measured.

1.3 Hum measurements shall be made at an axial distance of 12 in. between the diaphragm of the microphone and the grille of the speaker. Where difficulty is found in determining this distance, the position chosen shall be clearly described.

1.4 The mains used for the hum measurement shall contain approximately 3 % of 5th harmonic.

One possible method of fulfilling this requirement is first to eliminate all harmonics by using a low-pass filter, and then, by means of a synchronized motor-alternator (or equivalent valve device), to generate the necessary 5th harmonic frequency across a low resistance inserted in the filtered mains supply.

(2) Method of Measurement

2.1 The essential elements of the measuring apparatus are:—

(*a*) The calibrated microphone defined in Section 1.

(*b*) An amplifier detector system having the following characteristic including the microphone frequency-calibration:—

Frequency, cycles per sec.	*Gain relative to that at 25 cycles per sec.*
25	0
50	+ 12·5
100	+ 25
150	+ 31·5
200	+ 36
250	+ 40
300	+ 43·5
400	+ 48
500	+ 51·5
600	+ 52·5
700	+ 54
800	+ 55
Above 1 000	Less than — 10

The figures given in the right-hand column shall be subject to a tolerance of 2 db.

The meter used shall read r.m.s. values.

2.2 A practical method of realizing the above characteristic is first to build a flat system and then to insert, in a suitable part of the amplifier, an equalizer in the form of a tee pad, arranged to work between equal resistances, R_0.

The equal series arms of this equalizer are condensers of $(398/R_0)\ \mu$F, and the shunt arm is a resistance of $0\cdot117\,R_0$ in series with an inductance of $7\cdot17\,R_0\ \mu$H. The characteristic of this equalizer fits the characteristic quoted in par. 2.1 with a maximum error of 0·6 db.

Additional arrangements must be made to remove or suppress the frequencies above 800 cycles per sec.

(3) Method of Expressing Results

3.1 Hum level shall be expressed in phons by applying the following formula:—

$$L = 65 + 20 \log_{10} (V/pq) \text{ phons}$$

where L = required output level.

V = output voltage of the amplifier detector.

p = ratio of volts in the output meter to volts at the microphone at 400 cycles per sec.

q = sensitivity of the microphone in volts per dyne per cm² at 400 cycles per sec.

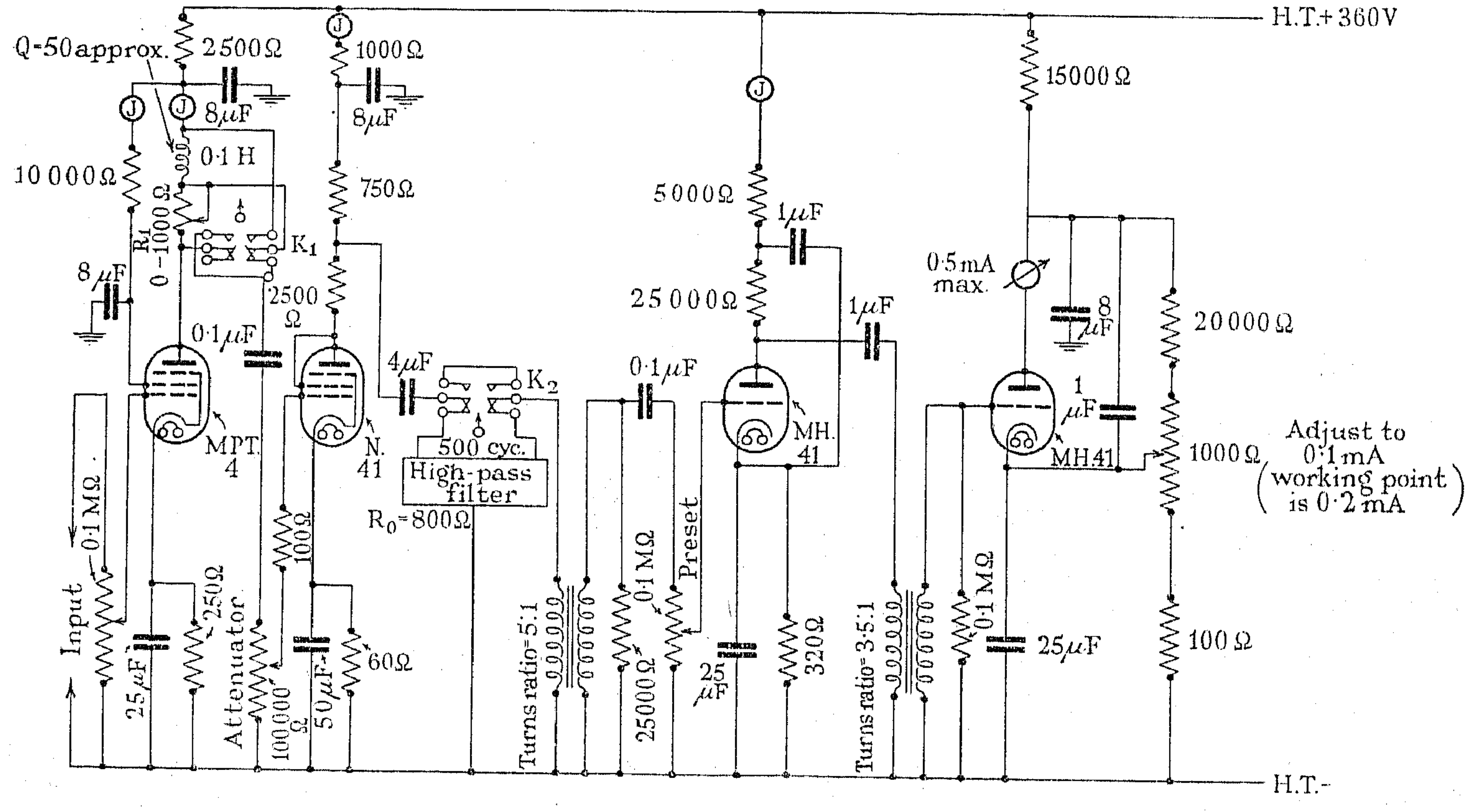

Fig. 1.—Distortion factor meter.

It is here assumed that the hum is a freely progressive wave remote from its source. The assumption is not necessarily valid and the conversion is therefore to be regarded as partly conventional.

The hum quoted for a radio receiver shall be:—

(*a*) The maximum hum obtainable, without carrier, with any settings of the controls; and

(*b*) The maximum hum obtainable for any input of any carrier between 0 and 1 volt and any setting of the controls.

The hum quoted for a gramophone shall be the maximum hum obtainable for any setting of the controls as the pick-up traverses a plain-cut 12-in. record.

In the above clauses, maximum hum shall mean maximum deflection of the output meter.

SECTION 5.—ACOUSTIC OUTPUT

(1) Conditions of Measurement

1.1 For the purposes of this Specification, acoustic output is considered in relation to the distortion factor of the (electrical) output. No attempt is made to relate acoustic output to acoustic distortion because of the complexity of the problem and the difficulty of interpreting the results.

1.2 Measurements of overall distortion of an open-aerial radio receiver shall be made with an input of 20 mV at 1 000 kc. modulated at 400 cycles per sec. to a depth of 60 % and applied to the receiver through the standard dummy aerial.

1.3 Measurements of overall distortion of a frame-aerial receiver shall be made with an input of 20 mV per metre obtained with the aid of the shielded coil as specified in Section 1 of Part I of this Specification. This input shall be at 1 000 kc. modulated at 400 cycles per sec. to a depth of 60 %.

1.4 Where a variable noise suppression control is fitted it shall be set until the sensitivity, as defined in Section 2 of Part I of this Specification, is a maximum.

(2) Method of Measurement

2.1 In principle, the method of measuring distortion is as follows:—

The output (fundamental + harmonics) is first amplified through an amplifier with a uniform frequency characteristic; the harmonic content (the fundamental being removed) is then amplified through an amplifier whose gain is proportional to frequency. If the amplitude in the first case is E_1 and in the second E_2, the distortion factor is given by the ratio $E_2/(2E_1)$.

A detailed circuit having the required characteristic is given in Figs. 1, 2, 3 and 4. With key 1 in the " up " position, the gain of the amplifier is constant for all frequencies; in the " down " position the gain is proportional to frequency. With key 2 in the " up " position the whole output is measured; in the " down " position the fundamental is suppressed.

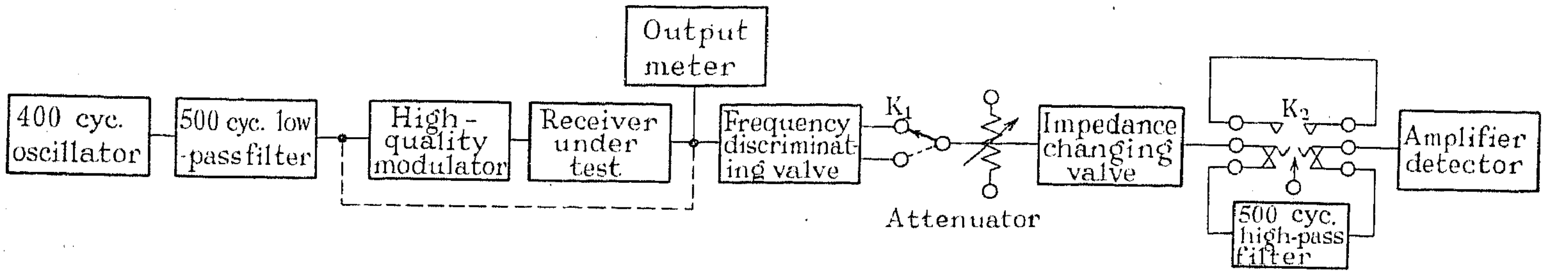

Fig. 2.—Block schematic of distortion measurements.

The apparatus is set up as follows:—

A pure tone from the oscillator is applied to the input of the apparatus through the low-pass filter; with key 2 " up," and with the input gain control and attenuator in convenient positions, the resistor R_1 (Fig. 1) is varied until the output reading is the same for both positions of key 1.

The purified input is then applied to the modulator, and the output of the receiver is taken to the input of the distortion-factor apparatus. With the attenuator at maximum gain and key 1 " down," the input gain control is adjusted to give the standard reading on the valve voltmeter. Key 1 is then moved " up," and the attenuator adjusted to give the same reading. If the gain given by the attenuator is now $1/x$ of its original value, the distortion factor is $1/(2x)$. The attenuator can therefore be calibrated directly in distortion factor.

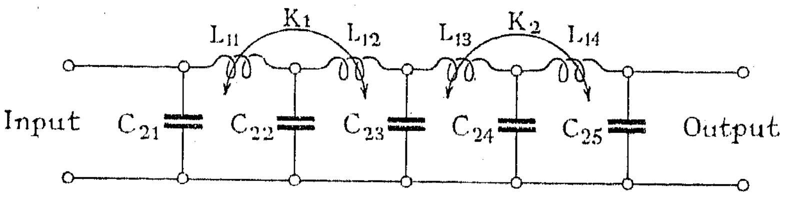

Fig. 3.—Low-pass filter for use with distortion-factor meter.

Element values, limits ± 1 %.

$L_{11} = L_{12} = L_{13} = L_{14} = 0{\cdot}908$ H.

$K_1 = K_2 = -12{\cdot}24$ %.

$C_{21} = C_{25} = 0{\cdot}399\ \mu$F.

$C_{22} = C_{24} = 0{\cdot}622\ \mu$F.

$C_{23} = 0{\cdot}798\ \mu$F.

$f_c = 500$ cycles per sec. $f_\infty = 800$ cycles per sec. $R_0 = 800\ \Omega$.

Attenuation at 400 cycles per sec. = 2 db.
Attenuation at 800 cycles per sec. = at least 80 db.
Attenuation at 1 200 cycles per sec. and above = at least 80 db.
} Q of coils = 20 approx.

Any other apparatus which fulfils the initial requirements can, of course, be substituted.

(3) Method of Expressing Results

3.1 A curve shall be plotted on plain-ruled cm. graph paper between acoustic output, expressed as a loudness level, and distortion factor.

Only distortion factors greater than 3 times the distortion factor of the modulation envelope of the signal generator shall be plotted.

The distortion factor shall be plotted as ordinate to a scale of 1 cm. = 0·01.

The acoustic output shall be plotted as abscissa, the scale being so chosen that a range of about 15 cm. is covered.

The curve may be plotted by taking the 400-cycles-per-sec. power, W_1, in the voice-coil or operating winding as parameter. The distortion factor corresponding to W_1 is measured as detailed above, and the acoustic output is calculated from the frequency response measurements as follows. The intensity level of the output, I_{W_1}, is calculated from the formula

$$I_{W1} = I_W + 10 \log_{10} (W_1/W)$$

where I_W is the 400-cycle intensity level referred to in Section 2, and W is the corresponding power in the voice coil. This intensity level is then converted to a loudness level by means of the Table given in Section 1, par. 2.03.

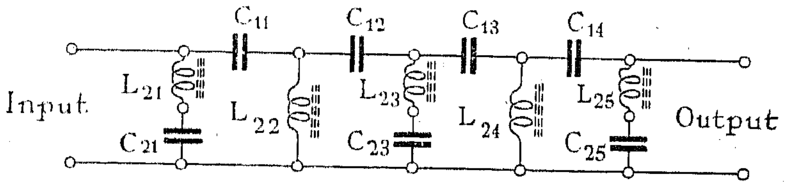

Fig. 4.—High-pass filter for use with distortion-factor meter.

Element values, limits ± 1 %.

$L_{21} = L_{25} = 0{\cdot}424$ H. $C_{11} = C_{12} = C_{13} = C_{14} = 0{\cdot}249\ \mu$F.

$L_{22} = L_{24} = 0{\cdot}127$ H. $C_{21} = C_{25} = 0{\cdot}373\ \mu$F.

$L_{23} = 0{\cdot}212$ H. $C_{23} = 0{\cdot}746\ \mu$F.

$f_c = 500$ cycles per sec. $f_\infty = 400$ cycles per sec. $R_0 = 800\ \Omega$.

Attenuation at and above 800 cycles per sec. is below 1 db.
Attenuation at 400 cycles per sec. = at least 80 db.
} Q of coils = 50 approx.

INTRODUCTORY REMARKS TO THE DISCUSSION BEFORE THE WIRELESS SECTION ON THE ABOVE R.M.A. SPECIFICATION

By STUART HILL, M.Eng.

AIM OF THE SPECIFICATION

Before it is profitable to discuss whether the specification now to be considered fulfils its functions, it is necessary to define with some nicety its aim, as conceived by its compilers, and its relation to the two most interested parties—those who make and those who buy.

Many views were expressed on this subject at an Informal Meeting of this Section on the 26th November, 1935. On that occasion two members of the Technical Advisory Committee of the Radio Manufacturers' Association referred to the present specification, then in preparation, and sketched the view which I now wish to make clear. The radio public is of course interested in overall performance, and a purchaser will ask his dealer such questions as how many stations he can receive on an indoor aerial, and whether he can separate his favourite foreign station from its neighbours on the dial. Quality, he will judge for himself. In all this there is no question of phons, decibels, frequencies, or other technical matters. We therefore feel that intelligibility to the lay public need not be a primary aim of the specification. Indeed, on the principle that "a little knowledge is a dangerous thing," it is probably best that it should be wholly unintelligible to the public. Nevertheless, there is some advantage from the layman's point of view in the existence of a standard specification for expressing performance, as it makes it just a little harder for an unscrupulous manufacturer to mislead a purchaser by publishing curves obtained under specially favourable conditions. Moreover, although a user may not understand the significance of the curves he sees, the knowledge that they have been obtained in accordance with a specification sponsored by a responsible body may serve him as a guarantee of good faith.

But although the specification may mean little to the public, the public must mean everything to the manufacturer. However technical the method of expressing results, the results expressed must be closely related to what the public hears. In this way, a manufacturer who obtains an improved performance figure for some item covered by the specification may feel assured that his customers will obtain a direct benefit. It is for this reason that the specification deals as far as possible with overall performance. There are several measurements which a design engineer will apply to his product as a matter of course—such as electrical response measurements—for which, however, the specification makes no attempt to legislate. The argument is that, from a user's point of view, it is a matter of indifference whether, say, a peculiarity in the speaker response is compensated for by an adjustment of the electrical response of the receiver, provided that the overall performance be satisfactory. Our standpoint may perhaps be made clear by saying that, while not seeking to enlighten an actual public, we have continually imagined our specification presented to a Utopian public or an ideal purchaser, capable of criticizing it.

Turning now to the relation of the specification to the manufacturer, it must be remembered that it has been prepared by manufacturers for manufacturers. It is not an attempt to instruct the radio profession, but to help those who make radio receivers. Without such a specification it is not possible for makers to discuss their products among themselves, since one maker's definition of selectivity is not the same as another's: with such a specification the way is opened to closer co-operation. An agreed specification may also help manufacturers who have to submit tenders to governmental or similar bodies. These bodies have hitherto made their own specifications, which manufacturers have been obliged to follow. It is hoped that, in future, it may be possible to persuade such customers to base their requirements on the present specification, to the benefit of both parties.

Those who have attempted to draw up, in committee, a technical document generally acceptable in a wide field, will realize how hard it has been to fulfil the aims set out above and will view our attempt charitably. Realizing that the specification must before all be a practical document which manufacturers can apply to existing products, we have frequently found it necessary to compromise between the ideal and the realizable.

SCOPE OF THE SPECIFICATION

The specification is intended to apply to as wide a range of types as possible, but some omissions have been inevitable. Thus receivers with variable reaction have been excluded because of the obvious impossibility of specifying exactly how the reaction shall be used. Again, when discussing hum the problem of legislating for direct-current mains was thought to be too complex to tackle at present. Fortunately, the use of alternating-current supplies is increasing, so that the omission is not very serious. Some of the tests specified are capable of extension beyond the scope explicitly mentioned. Thus acoustic-response measurements of an amplifier-speaker combination might be made in accordance with the principles laid down, audio-frequency inputs being substituted for radio-frequency inputs. The specification has been worded so as to be as elastic as possible, but, no doubt, cases will arise where it is difficult to interpret. In these cases, we rely on the manufacturer to trim the specification to his special needs, keeping as close as possible to the spirit of the text.

The specification is presented in two parts: the division into electrical tests and acoustic tests is clear enough, but the matter goes rather deeper than this. The first part deals with well-recognized tests which can be adopted by manufacturers without any revolutionary changes in their present practice. The second part deals with tests which are more controversial and involve more specialized plant and equipment. The parts have been separately issued in order that the more difficult matters of Part 2 shall not prejudice the acceptance of Part 1.

APPARATUS

The apparatus called for in the first part of the specification is such as all manufacturers are likely to possess, and the accuracies imposed are considered to be well within commercial possibilities. The equipment needed for the acoustic tests is more elaborate, the most important item being a heavily damped room. The construction of such a room is no easy matter, as there seems to be no standard procedure. It was thought at one time that the compilers might be able to specify a suitable room in detail, and they might perhaps have done so had it been merely a matter of specifying the size of the room and the details of the damping materials. The experience of the various authorities consulted, however, suggested that the structure of the chamber might introduce appreciable effects, and it was therefore decided to leave the construction open to the manufacturer. Possibly the present discussion will bring out valuable information on this point. What is aimed at is a room which will simulate free-space conditions within 2 or 3 db. over as wide a frequency range as possible.

The only other item of equipment presenting any difficulty is the set-up for measuring the acoustic response. The choice is again left open, so that manufacturers already possessing equipment shall not be obliged to change it. In its simplest form, the equipment may consist of a microphone, an amplifier, a heterodyne oscillator, and an output meter, but some elaboration of this basic form is desirable if measurements are to be made quickly. A recording drum driven synchronously with the heterodyne oscillator is very desirable. The output may then be passed through a logarithmic indicator and recorded directly on the drum. Alternatively, an attenuator may be used to keep a spot of light stationary on a scale, the readings of the attenuator being plotted against frequency; but this is a much slower process. Whatever method is used it is expected to be continuous, so that no sudden changes in output are missed. A further elaboration which may be found necessary in noisy locations, or where interference is troublesome, is a tuning device arranged to suppress all frequencies other than that being examined at a given instant.

For hum measurements a " weighted " amplifier is called for, but this should not present any difficulty to a well-equipped laboratory. The specification gives the constants of an equalizer, simulating the required loudness curve, which can be applied to a " flat " system. Engineers, however, will probably prefer to produce the weighted curve in other ways, such as using suitably-designed reactive coupling elements between stages.

ELECTRICAL TESTS

As it is presumed that those interested have studied the actual specification, it will not be necessary to go through it in detail, but the following notes on the individual sections will perhaps help to make clearer what was in the minds of the compilers. An interesting omission may perhaps be noted here. No attempt has been made to measure second-channel and other similar types of interference. The number of such types of spurious response is so many that they could hardly be dealt with individually, while to omit any one of them might lead to a receiver which shone according to the specification and was yet more prone to spurious response, from the customer's point of view, than another receiver which did not pass its tests so well.

Sensitivity

The method proposed for expressing sensitivity follows existing practice closely. Since a dummy aerial is used which may or may not simulate the customer's aerial, the figure obtained is a somewhat conventional one. It also suffers from the slight disadvantage that it is inverted, i.e. that a greater sensitivity is expressed by a smaller number. Nevertheless, the method is thought adequate to compare the performances of two receivers, or to indicate the success of the designer in covering the wave spectrum with uniform sensitivity.

Selectivity

Many manufacturers still measure selectivity by measuring the increasing input needed to give a 50-milliwatt output as the receiver is progressively detuned. The method proposed, however, although it involves two signal generators, has the considerable advantage that the conditions obtaining in practice are more nearly simulated. It is a method, moreover, which has been gaining ground for some time past among manufacturers.

Noise

The method here proposed of measuring noise by comparing it with a known single-frequency modulation is perhaps rather arbitrary. It is by no means certain that a higher noise figure will always correspond to a noise of higher " nuisance value." Nevertheless, experience has shown that, applied to an ordinary receiver, it gives a fairly good general indication of the noise. It will be noted that a 1 500-cycle modulation has been used in place of the 400-cycle modulation used everywhere else. By using the higher frequency it will not be possible to suppress noise at the tone control of the receiver, without affecting the 1 500-cycle comparison tone to some extent. The figure obtained will therefore be more independent of the position of the tone control than if the measurement had been made at 400 cycles.

Automatic Volume Control

This is a straightforward measurement, closely following current practice, and does not seem to call for any comment here.

ACOUSTIC TESTS

It was pointed out by Dr. Smith-Rose, at the Informal Meeting already referred to, that, ideally, all overall measurements of a receiver should be framed in acoustic terms. This view is in accordance with the aim of this specification as already set out, but we feel that to cleave too closely to this ideal would lead to practical difficulties in the present state of the art. We have, however, taken a step towards this goal. Thus we have made provision for an overall acoustic-sensitivity test, which, apart from the element of vagueness introduced by the dummy aerial, comes near to being an " air-to-air " test.

The following notes, amplifying the text of the specification, will perhaps bring out our intentions more clearly.

Frequency Response

Following the plan of making the tests follow as closely as possible what the listener hears, the compilers gave earnest consideration to a scheme for measuring the acoustic-frequency response in a chamber which should simulate ordinary living-room conditions. It was decided, however, that the difficulties of specifying the room and the correct microphone position in that room were almost insuperable, and that the results obtained would probably bear no more relation to what a particular listener might hear than those obtained in the much simpler " dead " room. With some reluctance, then, the live room was abandoned in favour of free-space conditions, or the more usual dead-room approximation to free-space conditions. The two series of measurements, namely axial response and 45° response, are also the remnants of a more ambitious scheme, involving integrated polar distribution, which was abandoned during the Committee's deliberations on account of the labour which such measurements would entail. The plotted curves show the relative response at different frequencies, and are also given an absolute significance by determining the intensity level of the zero line which passes through an averaged point in the neighbourhood of 400 cycles per sec.

Acoustic Sensitivity

The acoustic sensitivity is a calculated figure, which makes use of the 400-cycle intensity level, read from the zero line of the response curve, and of the electrical sensitivity measurements made in accordance with the first part of the specification. The inclusion of such a measurement means that manufacturers who go to the expense of an efficient loud-speaker may see the improved efficiency reflected in an improved sensitivity figure.

Hum

The measurement of hum presents special difficulties, due partly to the low levels encountered, and partly to the shape of the ear sensitivity characteristic at these frequencies. The method proposed is an objective method which seeks to measure the loudness of the hum on the assumption of a normal ear. It is open to the logical objection that the correct ear characteristic can only be chosen if the loudness to be measured is already known, but errors introduced through this cause are unlikely to be of any importance. A further theoretical objection is that the microphone is placed 12 in. from the receiver and therefore is not measuring a freely progressive wave as demanded by the theory of the calibration. More important than either of these points is the difficulty as to choice of mains. It is possible that a given receiver may pick up a harmonic of the mains supply through stray capacitance, and that the resulting noise may be more troublesome than the pure 50-cycle or 100-cycle hum. The use of a pure supply might easily mask this effect. On the other hand, it is obviously impossible to specify an impure supply in any detail. The injection of 5th harmonic is an attempted compromise on this point, about which it is hoped to gain experience from users of the specification.

Acoustic Output

The only ultimate criterion of output is that of noticeable distortion. Whatever measurements are made to determine it should be acoustic measurements, and should take account of harmonics and combination tones generated in the electrical circuits and in the loud-speaker. The compilers see no hope of specifying anything useful along these lines at present, and have been obliged to measure distortion electrically. The output corresponding to any distortion, however, is given acoustically. The ideal purchaser to whom reference has been made is supposed to choose a degree of distortion which he regards as tolerable, and to say "I can get x phons from this particular receiver, after which the set will overload electrically." No guidance can be given in choosing his limit of tolerable distortion, partly because it is not the function of this specification to fix any such limits and partly because in the present state of the art we do not know enough about the relation between harmonic content and the subjective appreciation of distortion. On this point the present discussion should be of considerable value. Although no accurate relations between distortion and harmonic production are known, there is good evidence that the higher harmonics are of more importance than the lower, presumably because there is a greater chance of the associated modulation products producing dissonance. For this reason, the usual r.m.s. measure of harmonic distortion has been rejected in favour of a weighted distortion factor, in which each harmonic is weighted according to its order, e.g. 3rd harmonic is reckoned as 3/2 times as important as 2nd harmonic. It is not claimed that this weighting is the correct one, but it is suggested that it will be easier for workers to base their experience on the weighted than on the unweighted factor. Moreover, this particular weighting lends itself to a fairly simple measurement by comparing the voltage developed across a pure resistance with that developed across a reactance which is equivalent to it at the fundamental frequency. Since the measurements involve a rather special set-up, the compilers have given the user some guidance in the matter by suggesting a circuit and specifying the details of the filters.

CONCLUSION

The compilers of the specification have applied the various tests to their own products and have obtained satisfactory agreement between measurements made on the same receivers in different factories. Satisfactory agreement has also been obtained between response curves obtained in various damped rooms and in the open air. It is hoped, therefore, that the specification, though necessarily imperfect, is a practical document which manufacturers can safely apply and on which governmental and other bodies may reasonably be expected to base their requirements.

It is not intended that the provisions of the specification shall be final, but rather that there shall be a period of probation, during which experience is accumulated by those who use it. It is hoped that manufacturers will co-operate in following the specification as closely as possible, for otherwise there can be no improvement. It is also hoped that when the compilers next meet to review and revise their work, they will be able to incorporate ideas brought forward during the present discussion.

DISCUSSION BEFORE THE WIRELESS SECTION, 3RD FEBRUARY, 1937

Dr. R. L. Smith-Rose: In Mr. Hill's introductory remarks it is stressed that the specification has been prepared by manufacturers for manufacturers. What is ultimately required, however, is an agreed specification, prepared by a representative body of not only manufacturers but also users and any independent authorities; in fact, we want a Standard Specification, possibly of the present type but sponsored by an independent responsible body. When I raised this matter at the discussion on Mr. Thomas's paper* in 1932, it appeared that my suggestion was a little premature. I am going to suggest that the time for the preparation of a Standard Specification has now arrived, seeing that the technique of broadcasting-receiver development has reached a more or less stable condition, particularly as regards the long- and medium-wave broadcast bands. Mr. Hill's remarks contain no indication that a Standard Specification is contemplated by the R.M.A., and his final sentences envisage the fact that, after accepting the criticism which this meeting and other publicity given to the specification may produce, the manufacturers will themselves consider whether it will be necessary for them to review and revise their own work. It is therefore for the manufacturers to take the initiative in this matter, and make a definite approach to the British Standards Institution.

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* *Journal I.E.E.*, 1932, vol. 71, p. 128.

Mr. M. G. Scroggie: With regard to what Dr. Smith-Rose has said about the proposed specification being sponsored by an official, independent body, it would be necessary to see that this proposal did not lead to undue delay. Some years ago, when ebonite panels were being standardized, by the time the specification was finished ebonite panels had long since become obsolete.

Referring to Part I, Section 2, paragraph 1.2 (long-waveband sensitivity measurements), I deprecate the choice of 200 kc. as a measuring frequency, because, unless one makes the measurements in a screened room, Droitwich is liable to leak through sufficiently to introduce difficulties owing to beat notes. The frequencies I have found most suitable in practice are 175, 225, and 275 kc., and these have the advantage of being evenly spaced over the scale. As regards the method of expressing the sensitivity results (paragraph 2.1), I take it that if a smooth curve is plotted the three specified points will not be assumed to be adequate data for the purpose.

In Part I, Section 3, of the specification it is stated " this measurement applies at present only to long- and medium-wave bands." Is there any practical objection to applying it to short-wave bands? I take it that it is intended later to extend this work more fully to those bands. I should like to suggest that it is better to express the results of selectivity measurements in decibels, instead of as " the strength in millivolts of the unwanted carrier." Decibels are more appropriate, seeing that selectivity is essentially a ratio; and this is implied by specifying 40 db. in paragraph 1.2. An additional reason is that the output power specified for this measurement is different from that for sensitivity.

In Part I, Section 4, paragraph (1.1), it is stated that the r.m.s. noise output is measured. I think that in almost all cases a metal-rectifier output meter is used, which of course does not measure r.m.s. values, and the error in the case of wave-forms such as are in view is considerable. In paragraph 1.4, why is 100 cycles per sec. selected for the filter? If it is to remove hum, this frequency is too low to do so effectively.

With regard to automatic volume control, the method described is open to the objection that if one starts with the maximum signal at 1 volt (incidentally, not all modern generators give a full volt), adjusting the output to a quarter of the nominal power output, the outputs at the smaller signals may be too small to measure; and this region of the curve may be important. Also the method fails to show whether the delay voltage is correct. The method that I have adopted is to begin with the smallest signal, with the volume control at maximum. Then, when the output reaches such a point that overloading is within sight, e.g. at one-quarter of the nominal output, the volume control is turned down until the output reading is reduced to one-tenth. If necessary, the process is repeated at a still higher signal voltage. This method sometimes gives apparent output figures considerably in excess of the nominal output for the receiver, which is an indication that the delay voltage is excessive. I think this point is rather important, because the ease of handling of the volume control depends very much on the range of volume available being correct.

I see there is no reference in the paper to quiet automatic volume control (Q.A.V.C.). I take it that the same method would be used, but I should point out that it is necessary, in plotting a curve of Q.A.V.C., to take it both with increasing and with decreasing inputs, because sometimes the curve forms a loop. The specimen curve* obviously shows the ordinate scale in volts instead of watts as specified.

With regard to Part II, in Section 1 it might be desirable to correct the expression " equally as loud as the signal," which is not good English. Also, with regard to the words " a note which an average listener judges to be equally as loud as the signal," surely this is a matter of enormous difficulty. If one has to compare a note of 1 000 cycles with one of, say, 50 or 5 000 cycles, it is almost impossible to judge even approximate equality of loudness.

I am glad to see that a real attempt has been made to tackle the measurement of hum. I also notice that Mr. Hill realizes the difficulties, e.g. that apparatus which works perfectly quietly on mains in one area may emit an intolerable hum elsewhere.

With regard to the measurement of acoustic output, it is difficult to comment on this in the absence of practical experience, but the specification seems to embody a courageous attempt to solve an extremely difficult problem. One realizes this when even the somewhat complicated method described yields only very partial data. The curves ought really to be extended to several other degrees of freedom—the whole audio-frequency scale instead of one point; the whole range of modulation depth instead of one point; and actual (reactive) loads instead of an artificial load. All this would naturally make the work far too laborious for practical purposes.

Dr. S. Whitehead: I should like first to refer to one or two details of technique in respect of which the specification may require modification.

The use of two signal generators in series involves some care, and the American specification—rightly, I think—leaves a good deal to the judgment of the operator. The British specification is more detailed, to the point of being somewhat restrictive, without dealing with all the difficulties which may beset the inexperienced. At the higher-frequency end of the band covered, the shielded-inductor method may be troublesome. Precision field-strength sets usually employ a direct coupling, but even so do not give very good comparisons. The E.R.A. have found it convenient to employ the radiation field of a loop carrying a known current for the calibration of receivers with frame aerials at higher frequencies.

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* Not reproduced in the *Journal*.

The possibility of avoiding the 1 500-cycle reference tone in addition to the 400-cycle reference frequency which itself is, I believe, mainly confined to radio-receiver practice, is deserving of consideration. The difference between the C.C.I. standard frequency of 800 cycles per sec. and the acoustic reference frequency of 1 000 is unfortunate, and any simplification of standards in other fields would assist the attainment of a single ultimate standard tone. Similar considerations apply to the assessment of a complex wave. Different circumstances require different types of weighting for various components, but in the absence of a comprehensive statistical analysis for the specific problem (such as the psophometric voltage defined for telephone practice by the C.C.I.) it would seem better to select such of the existing well-known weighting curves, of which there are several, as seems the most appropriate. For example, the weighting curve for measuring hum in the present specification corresponds closely to that adopted by the C.C.I. for disturbance to broadcast transmission over telephone channels, except that the curve for hum cuts off at 800 cycles per sec. Again, distortion appears to be assessed both by the "harmonic content," which is the same as the "harmonic residue," a term now of some years' standing, and also by a weighting proportional to frequency. The distortion of the electric supply is represented by the addition of 3 per cent 5th harmonic, which is somewhat arbitrary in view of the amount of work that has recently been carried out on this subject in Europe and America and in which the commoner types of distortion have been analysed and to some extent defined. One might envisage the expression of component quantities in such a way that they would represent a simple subtraction from or addition to the fidelity of the transmission as a whole, so that the special and subjective features of this particular service would be included in a single unit to which others could be related, as is attempted in telephone practice. Be this as it may, the question of appropriate standards supports the later intervention of the B.S.I., suggested by Dr. Smith-Rose, since the B.S.I. acts as the National Committee of the I.E.C., which, as a standardizing body, is attempting to organize some international agreement in the definition of acoustical and associated quantities, which are becoming of such importance in electrical engineering.

Finally, I hope that a section on the assessment and reduction of the liability of a receiver to interference will later be included in the specification. Screening, mains filters, design of aerials and earths, and many other features, can serve to reduce interference. At a time when specifications are in preparation for the reduction of interference from electric appliances and plant, corresponding directions to the users of this specification would have a valuable influence. Whatever the eventual amelioration of the situation may be by other means, it is probable that appropriate measures at the receiver will be necessary in some unfavourable cases, where the full advantage is desired to be taken of the progress in receiver performance, a progress to which this specification is itself a witness.

Mr. E. L. E. Pawley: It is an unfortunate fact that many listeners are quite as happy with very bad quality as they are with technically excellent quality. I think we must admit, however, that a discriminating listener has a right to a receiver which will come up to his requirements, and that all manufacturers would like to be in a position to say amongst themselves whether or not their receivers fulfil those requirements.

The ideal solution of the problem is to relate the radio-frequency energy in the aerial to the acoustic effect on the listener's ear, because his ear and the corresponding effect on his brain are the only things that ultimately matter. There are, however, two very great difficulties in making these acoustic tests. One is the cost of the damped room, which may have been rather under-estimated, because it is difficult to construct a satisfactory damped room for measurements going down to 150 cycles, and to get down to 30 cycles is extremely expensive, if not impossible. The other point is that it is necessary to allow for the acoustics of the listener's room, which are in general not known. This is particularly difficult in the case of broadcasting receivers used in schools, where there is a considerable distance between the listeners and the loud-speaker, and the proportion of reverberant to direct sound is unusually high; and consequently the total spherical output of the loud-speaker may be considerably more important than the output in one particular direction. The specification tries to get over this difficulty by stating that the output shall be measured in two different directions, namely one along the axis and another at an angle of 45° to it; but I am not sure that this provision will be adequate to cope with cases, such as I have mentioned, of broadcast receivers for use in schools.

It is interesting to compare the methods given in the specification with a method which has lately appeared in one of the French technical papers, and a similar one which has recently appeared in the German technical Press. In general the fundamental theory is very similar, but I have noticed one or two differences. In the first place, the average listening level for the sensitivity tests is taken in both the foreign methods as between 60 and 74 phons, whereas the R.M.A. specification mentions 50 phons. Another point is that the depth of modulation which is taken as standard in the R.M.A. method is 30 per cent, whereas the French prefer to use 80 per cent—probably unwisely!

There is one respect in which the specification might possibly be simplified. The formula given in Part II, Section 3, for converting the measurement of electrical sensitivity into the figure for acoustic sensitivity is not quite the sort of thing which the average engineer would use if he could help it. Would it not be very much simpler for the average man who is making a test on a broadcast receiver just to repeat a portion of the frequency curve under the conditions which are specified for the measurement of sensitivity, so that at one stroke he gets a final figure for the result instead of having to combine together three different factors in order to arrive at it?

I should welcome some more definite recommendations on several important subjects which are not adequately treated in the specification. The first is second-channel interference, which can be very important, especially in short-wave reception. The second is the possibility of non-linear distortion in the loud-speaker, which is not covered because it is very difficult to measure. I feel, however, that an attempt should be made to include it, bearing in mind the possibility of difference-tones and modulation products being actually of more importance than the harmonics which are measured in accordance with the specification. The third is the stability of the receiver and the accuracy of the frequency calibration.

There must be a certain reluctance to specify definite limits for individual characteristics when the overall performance of a receiver is an unknown combination of them, but I think we can assume that if all the receivers produced conform in every respect to a certain specification, and if we can say that 99·9 per cent of the people who purchase them and use them in their rooms, whatever they may be like, are satisfied, then we may consider that the specification is a satisfactory one.

Dr. L. E. C. Hughes: The figure of 50 milliwatts for the standard output seems low for present-day broadcast receivers, battery sets with an output as low as 400 milliwatts being rare. Also the output impedance of the signal generator is assumed to be so low that the output voltage in the specified measurements is independent of the impedance of the load. In rating the power output of radio receivers, what percentage harmonic distortion is taken? In Class-B battery sets the harmonic distortion generally drops to a minimum which may be greater than the accepted harmonic distortion for rating purposes.

In measuring receiver noise the output meter must be a square-law rectifier for the specified indications to be made. If the meter is of the metal-oxide rectifier type, which indicates the average of an applied wave-form, the indication with the two powers, one a 1 500-cycle tone and the other random noise, may not be the sum of the two powers measured separately. A simple calibration or calculation will, however, obviate this disadvantage. The zero of the British Standard phon scale is 0·200 millidyne (r.m.s.); the American standard is 0·204, but the difference is acoustically negligible. The level to be taken, i.e. 50 phons, is certainly too low. The level of my lecturing voice (average continuous r.m.s.) in a medium-sized classroom is about 84 phons, the occasional peaks reaching 91 phons. Allowing only 10 db. drop for a conversational level, this agrees with the figure (74 phons) given by the German specification as representative. With the normal technique of control, music will probably rise occasionally to 80 phons, which is not unreasonably representative of a large orchestra. The distortion factor, in taking into account the order of the harmonic, is a great improvement and will indicate a truer measure of amplitude distortion experienced with Class-B and pentode output stages, the use of which has greatly deteriorated the general average of reproduction obtained nowadays.

A more convenient way of expressing the amplitude distortion is to use the number of decibels the weighted power of the harmonics is below the wanted fundamental. This is representative of the blur level, i.e. the level of those alien frequencies which blur the required frequencies. The blur levels so obtained could then be compared with the levels obtained subjectively with other types of blur, such as are provided by reverberation. The extent to which a given distortion factor or blur level degrades the quality of reproduced sound can only be learned by subjective tests. Insufficient tests have been made to indicate the relative amount of the several classes of blur which are just detectable, or how much of one is equivalent to a given amount of another.

With regard to the radiation of sound from a loud-speaker, some experiments I made 10 years ago indicated that the response of a coil-driven cone mounted in a large baffle in a direction making 45° to the axis is substantially equal, within a few decibels, to the mean spherical response, which determines the average reverberant intensity in an enclosure and which determines the loudness except in the vicinity of the loud-speaker and for high frequencies, which are sharply focused into a beam. In expressing response curves, the ratio of the scales should be stated so that one can learn the meaning of response shapes by experience; 20 db. per cycle (i.e. a frequency multiple of 10) is convenient and is not acoustically misleading.

The Apparatus Approval Sub-Committee of the Central Council for School Broadcasting agrees with me that the ultimate test is a listening test under standard conditions, but appreciates the present specification as providing data, when it is used, respecting the standard to be expected in receivers for school purposes. It is unfortunate that the public never has the opportunity of hearing radio receivers under standard and strictly comparable conditions, so as to assess practical values. It is hoped that the application of the specified tests will indicate in some way a new commercial valuation of radio receivers for broadcasting.

Mr. C. N. Smyth: While I am appreciative of the value of the specification I am not quite certain that the tests for selectivity and sensitivity bear the simplest possible relationship to what the user hears, and I wish to suggest two small modifications which I think would overcome any criticism.

First, if according to the specification the sensitivity of a receiver is 1 or 2 microvolts the application of such a signal to the receiver might reasonably be expected to be able to produce an audible and intelligible output; but I do not think that in every case it would, because 50 milliwatts is frequently too small in comparison with the noise level of the receiver, or the input is too far below the level at which the muting action is released. I would prefer for these reasons to see the sensitivity measured as the input required to produce one-quarter or one-half the maximum rated output of the apparatus.

Secondly, with regard to the measurement of selectivity, in the past engineers have had to contend with four difficulties: (*a*) During the period of measurement the detector was not operating under the conditions obtaining in practice. (*b*) The A.V.C. action was liable to influence the results. (*c*) The audio-frequency filtering in the receiver was not taken into account in the measurement. (*d*) Effects of loud-speaker response on the effective selectivity were not included. Part 1 of the specification is not concerned with acoustic tests, and the new method of measurement overcomes the difficulties under (*a*) and (*b*) but does not take into consideration the fidelity of the audio-frequency stages of a receiver, where an appreciable fraction of the selectivity of a modern receiver is frequently located. I think, therefore, that it would be better, when measuring selectivity in the manner described in the specification, to have both signal generators unmodulated and to obtain the standard output at the frequency of the beat note between the two incoming signals, rather than to measure only the cross-modulation component.

Mr. A. J. Gill: I quite agree with Dr. Smith-Rose that what we must ultimately hope for is a British Standard Specification. The subject of this discussion is "A Specification for Testing and Expressing the Overall Performance of Radio Receivers," and it really only applies to one special type of radio receiver, namely a sound-programme receiver. Mr. Hill hopes that the specification will be used by Government Departments; but the specification only specifies the selectivity between 150 and 2 000 kc., i.e. in the broadcast band, and Government Departments do not buy large numbers of broadcast receivers. Also in Part I, Section 2, it is said that measurements shall be made "at one frequency in the middle of each of the accepted short-wave bands." I would point out that the bands quoted in the specification are accepted for broadcasting only.

There is apparently nothing in the specification which calls for measurements to be made, on superheterodyne receivers, at the intermediate sideband frequency. The curves only show a frequency about 15 kc. off the carrier for the maximum measurement. Some measurement ought to be made on superheterodyne receivers at the intermediate frequency, because I have known such receivers to be useless owing to the fact that an intermediate frequency had been selected which happened to coincide with the carrier frequency of some powerful station, and, as these sets were insufficiently screened, that frequency went through and swamped the rest of the output.

With regard to the remarks that have been made about measurements in reverberant rooms, one cannot blame a loudspeaker for imperfect performance in a reverberant room, and while we have reverberant rooms we shall have to put up with this difficulty. After all, we tolerate speech and music originating in such rooms, although such sounds must also be subject to distortion due to reverberation.

Mr. R. M. Barnard: The specification does not mention methods of measuring phase-shift. This measurement is important for two reasons: First, the output of a radio receiver which is used for sound reproduction always contains a certain amount of harmonics, and the importance of these harmonics depends to a certain extent on their relative phase. Secondly, in receivers for television purposes (presumably the specification would cover that type of receiver) phase is of very great importance, and I think that in a future edition of the specification a method should be suggested whereby the phase-shift through a radio receiver could be measured, particularly over the frequency band required for television.

A method of measuring phase-shift has been described by Peterson, Kreer, and Ware,* but in their case the phase-shift had only to be measured at one frequency; that is to say, it was only the comparatively simple problem of measuring phase-shift through an amplifier. In the case of a radio receiver the problem is considerably more difficult because this incorporates a frequency-changing valve, which, in theory, produces an output containing frequencies of the same relative phase as the original sidebands.

I think that in addition a figure should be suggested for the degree of phase-shift that is permissible through various types of radio receiver, depending on the purpose for which each is intended.

Mr. W. K. Alford: I have been asked to make a few remarks on the general attitude of the Army Signals authorities to the application of the specification.

After examining it fairly comprehensively we find we can give it our general approval. Unfortunately, a specification must be a very powerful weapon for both the Army authorities and the manufacturers, but it is the desire of the former that it should be a means of bridging over the difficulties which lie between the production as understood by the manufacturers and the special requirements which the Army must, of necessity, call for.

As regards selectivity, the Army requirements are vastly different from those necessary for ordinary broadcast reception, and we should require a much firmer qualification of the conditions of selectivity as outlined in the specification. In other words, we should call for a specification of the "cut-off" slope of the resonance curves in some arbitrary units such as decibels per kilocycle per sec., and we should require to know what happens outside the immediate neighbourhood of the resonance region. Further, the use of two signal-generators for the determination of selectivity would not be viewed with approval by the Army authorities. Another important point which would require attention is that a figure of sensitivity and selectivity would have to be arrived at for C.W. working as distinct from radio-telephony. As regards second-channel and other spurious forms of interference, here again considerable care is called for in drawing up the specification so that these points may be adequately covered. This matter assumes considerably greater significance when it is realized that in the future the use of the double superheterodyne will probably become more common, and in such a circuit there are great possibilities of a multiplication of spurious heterodyne frequencies. Frequency stability (particularly high-frequency) would also require qualification. Army wireless receivers are in most cases battery-driven, and most careful scrutiny is required to see that complete stability of performance is assured under all conditions. The calibration accuracy must be of the highest order for signalling purposes, and a definite standard of accuracy of calibration would be called for and expected.

Mr. Charles Holt Smith: The first thing that strikes me in reading this specification is the indecision regarding the accuracy with which the measurements are to be made. For example, in Part I a signal generator is called for with an accuracy of ± 10 per cent, whereas in Part II a signal generator is required of an accuracy of ± 20 per cent. Also in Part II, Section 1, a microphone is required to be calibrated to an accuracy of 10 per cent, whereas in Section 2 it is stated that the microphone in conjunction with an amplifier should be accurate to 0·5 db., i.e. 5 per cent. Again, in Part I we find that when we are measuring the sensitivity of a frame-aerial receiver the coupling coil which is placed near the centre of the frame must have an impedance of, say, 20 times the open-circuit impedance of the signal generator; remembering that the output impedance will be resistive, this imposes an accuracy of $\frac{1}{8}$ per cent. In the same paragraph a statement occurs with which I cannot agree. It reads: "It is essential that the shunt capacitance reactance of the coil and its connections shall be large compared with the inductive reactance." To get a minimum of error, that shunt capacitance reactance must be equal in magnitude to the inductive reactance. In fact, so long as the shunt capacitance reactance is not less than half the inductive reactance the error will be no greater than it would be if there were no shunt capacitance there at all.

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* *Bell System Technical Journal*, 1934, vol. 13, p. 680.

Again, when making distortion measurements we are instructed to insert a filter between the audio-frequency oscillator and the modulator. The performance of the filter is specified to be such that the second harmonic of the oscillator is attenuated by at least 80 db. Since the second-harmonic level in the oscillator output circuit can hardly fail to be less than -20 db. relative to the fundamental, the overall accuracy is in this case, by implication, 0·001 per cent!

I should like to express regret that the term "automatic volume control" has been perpetuated in the specification. "Automatic gain control" is more precise, I consider.

With regard to the distortion factor (Part II, Section 1), I should like to ask for an explanation of the factor $\frac{1}{2}$ which multiplies the expression defining this factor.

Finally, Part II, Section 5, states "A curve shall be plotted on plain-ruled cm. graph paper between acoustic output, expressed as a loudness level, and distortion factor." Unfortunately, however, the curve given at the end of the specification expresses an intensity level instead of a loudness level. I should like to know which one of these results really should be expressed. I notice that these tests should be made with a modulation percentage of 60, a figure which I regard as undesirably low. It would, for example, fail to reveal the obvious superiority of a diode as compared with a leaky-grid detector.

Mr. E. W. D. Negus: I do not consider that the specification covers every measurement necessary for a complete knowledge of a radio receiver, and I think that the methods of making measurements should be laid down very precisely. For example, I should like to see the measurement of padding errors included in the specification. One of the difficulties encountered when taking padding curves is the necessity to tune a circuit which has an asymmetric response. There are various ways of tuning such a circuit, but all are compromises. The thought induced by new methods of measurement may lead to considerable improvements, and the time would appear to be ripe for a series of carefully-prepared discussions upon the difficulties met with in making certain measurements, because it is apparent that few workers assess the same circuit in the same way.

For the measurement of selectivity I agree with Mr. Alford that it is undesirable for many reasons to use two standard signal-generators. Selectivity is required in a receiver to stop an unwanted station interfering with a wanted station, and the interference obtained I divide into two sections. One I call "side-band splash," and this occurs at frequencies from 1 000 to 3 500 cycles per sec. The other, for which I reserve the name "monkey chatter," occurs round about 500 cycles per sec. This is a purely personal division, which I have not seen stated previously. Comments on it would therefore be interesting.

Mr. Scroggie objected to the use of 200 kc. as one of the measuring frequencies, because he had met with interference from Droitwich; I would inform him that I have had no difficulty at all with interference from Droitwich at a place where the field-strength measured in the open is 20 millivolts per metre.

Mr. S. Hill (*in reply*): I agree with Dr. Smith-Rose that the time is ripe for a comprehensive specification sponsored by the British Standards Institution. The Radio Manufacturers' Association has borne this in mind and intends, when satisfied that the present specification has reached a stable form, to present it to the B.S.I. as the basis for such a comprehensive specification. Mr. Scroggie thinks that this work is likely to take a long time. This is a good reason for issuing the R.M.A. specification immediately so as to tide over the period of delay.

Mr. Scroggie notes that difficulties may arise with the 200-kc. measurement. It is expected that where such difficulties arise the manufacturer will find a solution which adheres to the spirit of the specification. In this instance a note will be added permitting the use of convenient adjacent frequencies. Where a smooth curve is drawn, additional frequencies must be used in any case, and the difficulty hardly arises.

The reason for limiting certain measurements to long and medium wave-bands is that the technique of short-wave measurements and the apparatus available were not considered to be sufficiently advanced at present. It may be feasible to include short waves in later issues.

The suggestion for measuring selectivity in decibels is quite sound, although it is perhaps open to the logical objection that decibels imply, by definition, a ratio of power, whereas we are here concerned with voltages only, the input impedance not being explicitly concerned. However, the definition given was thought to be more acceptable to manufacturers and also to bear the simplest relation to what interests the user, namely the strength of interfering signal which can be tolerated without audible confusion.

The point about the use of an r.m.s. meter for noise measurement is also sound. The definition really implies the use of such a meter, but in case of hardship the use of a rectifier instrument would be tolerated as not likely to introduce serious error.

It is agreed that there may be appreciable audible components of hum above 100 cycles per sec., but it is highly improbable that these would affect the meter visibly.

In his remarks on A.V.C. and Q.A.V.C., Mr. Scroggie opens a controversial question. The A.V.C. test quoted was primarily framed to cover the performance of an

A.V.C. system under the conditions which most often apply, viz. the reception of signals of the order of 1 mV, which will be attractive by being free from interference, but may be marred by fading. Any test intended to cover all the design points would have to go much further even than Mr. Scroggie's suggestion. The form of curve suggested by Mr. Scroggie has been considered but is felt to be open to objection. Admittedly the R.M.A. test calls for the measurement of a smaller output (though it is of the same order as that given by the proposed method), but both methods necessarily lead to a range of outputs which is too wide to be measured on a single instrument. The R.M.A. prefers to extend the range of the meter by the conventional method of scale multipliers, believing this to be much less vague than the process of resetting the receiver controls advocated by Mr. Scroggie. The point about the 1-volt output of signal generators is worth further consideration.

No test is proposed for Q.A.V.C., because this is only one of several methods used to achieve " quiet tuning." Other common methods are the use of limitation of the ultimate sensitivity, and the use of muting circuits controlled by selective discriminating circuits; there are also various mechanical methods. It would be difficult to devise tests which would distinguish these devices from the user's point of view, and the Committee therefore preferred to concentrate on tests directed towards the more important parameters of the receiver.

The specimen A.V.C. curve should, for consistency's sake, show an ordinate scale of watts, as Mr. Scroggie suggests, rather than of volts.

It is agreed that " equally as loud as " is probably scientific jargon rather than good English. I have striven to achieve clarity rather than an academically correct style; nevertheless, slovenly writing is to be deprecated and, if good dictionary authority can be found to support Mr. Scroggie's contention, the phrase will be amended. It is, of course, agreed that the loudness comparison of sounds of different characters is a matter of great difficulty. Harvey Fletcher and other workers, however, have made these comparisons once and for all and issued frequency-loudness-intensity curves which are generally accepted. These curves have been used in designing the weighting of the amplifier, so that no further subjective judgments are involved in the tests.

The difficulties of using signal generators in series are appreciated, but a detailed discussion of how to deal with such difficulties would, I think, be out of place in the specification and difficult to make comprehensive. In specifying the method the R.M.A. relies on the experience of its members who have applied the specification successfully to commercial receivers.

The multiplicity of frequencies commonly used as standards is a great pity. I can only say that these different frequencies have been used as sparingly as possible. There seems to be no hope of standardizing weighting curves, as each weighting is really only suitable for one type of sound and one loudness. Hum in a radio receiver is so specialized a noise that it is unlikely that there will be any clash with such a body as the C.C.I. or with those bodies concerned with measuring industrial noises.

The 3 % of 5th harmonic is certainly arbitrary, although it bears some relation to analyses of the various mains supplies available to the manufacturers who drew up the specification. It is not to be thought that 5th harmonic is specially prevalent. It was included to detect a particular type of hum often present in radio receivers and missed by 50- or 100-cycles-per-sec. measurements, namely the type of hum introduced by pick-up, within the receiver, by an unscreened grid lead. Lower harmonics than the 5th would not show up this defect, and higher harmonics are not sufficiently prevalent. Dr. Whitehead hopes to see the question of reduction of interference dealt with in later issues. The R.M.A. feeling, however, is that the question is best dealt with in an entirely different document and in co-operation with other bodies.

The average listening level of 60 to 74 phons quoted by Mr. Pawley is probably about normal; our own tests seem to suggest about 65 phons. The figure of 50 phons was chosen as a round number which would certainly not cause overloading in any receiver. It has much the same status as 50 milliwatts in the electrical tests. We hold that 80 % modulation is inconveniently high in the present state of the art of manufacturing signal generators. The R.M.A. is aware that such depths occur in practice, but it would like to see administrations accepting about 30 % as an average depth of modulation.

The formula given in Part 2, Section 3, appears more fearsome than it really is. Thus the expression $10^{(I_w - I_0)/10}$ is the conversion of a difference in decibels $(I_w - I_0)$ to its corresponding power ratio. It may be evaluated at once from a table of antilogarithms or by a single setting of a slide-rule. To repeat a portion of the test, as suggested, would be a more lengthy procedure than to make a slide-rule calculation.

It must be admitted that the various subjects mentioned by Mr. Pawley ought to be included in an ideal specification. The R.M.A. specification does not claim to be ideal. As pointed out in my opening remarks, it is written by manufacturers for manufacturers and, although criticism is welcomed and has been of use in modifying some of the detail, the general outline remains and represents all that the manufacturers are prepared to do at present.

In reply to Dr. Hughes the figure of 50 mW is certainly low, and it was specified because it is an accepted figure. Where it causes any difficulty, the specification permits measurements at higher levels together with a linear reduction. I cannot agree that battery sets with an output of less than 400 mW are rare. Receivers with an output of about 200 to 250 mW are very common.

Dr. Hughes asks what percentage of distortion is taken in assessing the output of a receiver. The specification has carefully avoided quoting any such figure: it is concerned only with the methods of measurement and the expression of results, not with performance limits. It is true that the nominal output of the receiver is referred to, but the margin allowed is so wide that no difficulty is likely to result whatever system of arriving at the nominal output be used.

Dr. Hughes is surely misinformed about the definition of the British Standard phon. The figure is 0·204 millidyne. It is agreed of course that the difference between this and Dr. Hughes's figure of 0·200 millidyne is negligible in practice.

Dr. Hughes's figures for practical loudness-levels do not accord with the Committee's experience. Measurements have been made in school class-rooms under a variety of conditions and for male and female voices. An average of 65 phons seems to be a suitable figure for voice levels. No tests were carried out on music, but I suggest that a level of about 80 phons would be normal. Dr. Hughes agrees with this latter level of 80 phons but seems to state that his voice (84 phons) is louder than an orchestra. Actually a real orchestra would be much louder than this. I have heard Tschaikowsky at something approaching the threshold of feeling!

In reply to Mr. Smyth there are difficulties about the use of one-quarter of the nominal output of a receiver in place of 50 mW. Firstly the nominal output is a figure which is not definite to within 50 %, and, secondly, the sensitivity figures would include A.V.C. performance and so give a misleading result.

The method of selectivity measurement proposed by Mr. Smyth is supplementary to the one specified. If extreme selectivity were obtained in the tuned circuits of a receiver and the frequency characteristics were compensated in the audio stages, the R.M.A. method would indicate a high selectivity corresponding to the fact that no speech or music would be heard from an adjacent station, whereas Mr. Smyth's method would indicate a much lower figure corresponding to the fact that there might be an appreciable monkey-chatter. It is debatable that the selectivity of a receiver should involve both measurements. The compilers, however, were unwilling to have two selectivity figures and, in choosing the method they specify, they were expressing the view which they believe is generally held by the public, namely that music-with-the-news is the more objectionable result of poor selectivity. "Monkey-chatter" can be dealt with, to some extent, by using the tone control, but, except in reaction sets, which are excluded from the R.M.A. specification, the other type of selectivity is in the hands only of the manufacturer. It may be added that for many listeners (such as, for example, the schools) selectivity means the ability to clear the London Regional of the National Programme. The beat-note method does not deal with this problem.

Mr. Gill's remarks suggest that the title of the R.M.A. specification should be changed from ". . . Radio Receivers" to ". . . Radio Broadcast Receivers." This suggestion is accepted by the Committee. Perhaps when the B.S.I. draws up the comprehensive specification, referred to earlier, it will be possible to extend the range to cover other than broadcast receivers.

Mr. Gill refers to the question of I.F. response. The Committee at one time drafted a section dealing with spurious responses. There were, however, so many of these, and it was obviously so invidious to omit any of them, that it was decided not to include spurious response at the present.

In reply to Mr. Barnard, phase-shifts are, no doubt, of growing importance. At present, however, only a small minority of users is interested, and it is felt that the time is not ripe for treating them. A user with a special interest in phase-shift would, no doubt, study the problem on two or three types of receivers, by means of a cathode-ray oscillograph, and choose that type which best met his special requirements.

Mr. Barnard should note that if phase-shift were eliminated altogether from the receiver, it would still creep in at the loudspeaker. Of course, the question becomes of more importance for television, but the specification is not intended to cover television receivers.

Mr. Alford's remarks are of great interest, since they represent a viewpoint necessarily different from that of radio broadcast receiver manufacturers. Mr. Alford certainly shows how wide a comprehensive specification would have to be. I only claim that we have laid down a skeleton and filled in such of the detail as is of interest in our own field. No doubt, allied specifications could be written by other interested bodies with applications in special fields. A specification which embraces all radio receivers may perhaps never be written. This is why we propose to adopt Mr. Gill's suggestion and strictly qualify the words "Radio Receivers" in the title.

As a result of Mr. Holt Smith's criticism of the accuracy limits imposed, the Committee has carefully reconsidered these and has changed some of them. Others, however, stand in spite of the criticism. Thus there is no real inconsistency in quoting a limit of 10 % for the signal generator in Part 1 and 20 % in Part 2. This merely means that for the measurements of Part 2 it is not necessary to bring out the best signal generator.

Again, although a figure of 20 times is mentioned, in parenthesis, for the ratio of the coil impedance and generator impedance, all that is really specified is that either open-circuit conditions may be assumed to hold or a suitable load correction shall be applied. If source and load are truly in quadrature it is true that open-circuit conditions apply with a relatively small ratio. Twenty times would only be necessary in the extreme case of a generator which, with its connections, was mainly inductive. Another instance in which Mr. Holt Smith has read into the specification more definiteness than was really intended is the attenuation of the low-pass filter. The attenuation of "at least 80 db." was the test figure of the actual filter used in the tests. It is only claimed for this circuit that it is one of many possible circuits having the required characteristics.

Mr. Holt Smith's contention that shunt capacitance across the coil will not introduce error is, of course, correct if it can be assumed that the capacitance is lumped. The Committee had in mind the possibly disturbing effect of distributed capacitance. However, the case for retaining the warning is not very strong and it has since been omitted.

Mr. Holt Smith is, however, quite right about the imposed accuracy of 0·5 db. for microphone and amplifier. The intention was to correct the device to within 0·5 db. on the assumption of a known microphone characteristic. This is not clearly expressed, however, and the statement will therefore need correction.

The term "automatic gain control" is certainly more accurate than "automatic volume control." For that matter, the knob known as the "volume control" should also be called a "gain control." The Committee, however, sees no likelihood of the term "A.G.C." becoming popular, nor does it feel it a duty to reform the standard nomenclature of receivers and components. One might waste much energy deciding between such terms as "valve" and "vacuum tube"!

The factor "$\frac{1}{2}$" introduced into the definition of distortion factor has somewhat the significance of the factor "10" introduced into the definition of the ampere. It is intended to make the range of values dealt with in practice compare with the percentage figures hitherto used, thus minimizing the shock which users might feel in changing from one standard to the other. This may be heresy academically, but we are concerned with the practical acceptance of the specification by manufacturers.

It is true that the present distortion (acoustic output) test takes no account of the distortion produced in the radio-frequency and detecting stages of the receiver. But the test proposed by Mr. Smith would introduce harmonics produced in the radio-frequency and the audio-frequency parts of the receiver with a random phase relationship, and would result in confusion.

Some members of the R.M.A. have sought to overcome the difficulty by carrying out the following tests:—

(*a*) The test called for in the specification, to show the audio-frequency distortion;

(*b*) A test under similar general conditions, but with low and constant audio-frequency output, the depth of modulation being varied; and

(*c*) A repetition of test (*b*) for a range of radio-frequency inputs.

These tests give a more complete description of the receiver than does the R.M.A. test, but the complexity of the tests and the scarcity of suitable signal generators force the Committee to be content with the present specification, which, after all, is mainly intended to combat the normal meaningless and often untrue claims of "undistorted watts output."

In reply to Mr. Negus it is agreed that there are many omissions in the specification. Some matters have not been treated because of difficulties of measurement or interpretation; others because of the difficulty of being sufficiently exhaustive; others again—and padding errors are probably in this class—because they do not seem important factors, from the users' point of view, in assessing the overall performance of a type of receiver. Manufacturers should take care of such matters as part of their routine testing practice.

It may, perhaps, be emphasized again that the specification is presented largely as a piece of information showing what the manufacturers intend to do. Possibly other bodies may be moved to write similar documents covering some of the matters we have not treated. Probably, also, the scope of the present specification will be enlarged as experience is accumulated.

Mr. Negus points out that there are at least two types of selectivity. The feeling of the Committee on this point is expressed in the reply to Mr. Smyth.

VECTOR DIAGRAMS OF POLYPHASE SYNCHRONOUS MACHINERY*

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(*Paper first received* 9*th April*, 1935, *in revised form* 4*th August*, 1936, *and in final form* 2*nd January*, 1937.)

SUMMARY

The paper is concerned with the theory and application of a vector diagram for synchronous machines which is expressible in terms of coefficients that are easy to measure if a source of large wattless currents is available.

This vector diagram is compared with the diagrams in general use, and some reasons are advanced, in regard to the latter, for the discrepancies between predicted and observed results which may occur when they are applied.

The theory of the diagram and the tests necessary for the definition of its coefficients are considered for the cases of a salient-pole and barrel-rotor machine on infinite busbars and on bars of varying voltage, and it is shown that, in general, good agreement is obtained between the predicted and observed results for regulation and load-angle shift.

In the Appendix a brief outline of the mathematical theory is provided.

LIST OF SYMBOLS

v and V = applied voltage.
E' = generated e.m.f.
E = e.m.f. equal and opposite to E'.
Φ = flux linkages, in volt-seconds.
i and I = root-mean-square armature current.
I_1 = root-mean-square armature current surrounding inter-polar space.
I_2 = root-mean-square armature current surrounding polar axis.
I_f = field current.
I_{f_0} = field current for given open-circuit voltage.
I_{fa} = armature equivalent of I_f.
I_0 = armature equivalent of I_{f_0}.
θ = load angle.
α = power factor angle.
l = leakage inductance of armature phase, considered as being independent of the position of the polar axis.
L = coefficient of self-inductance of armature phase for symmetrical machine.
κ = flux linkage per phase due to flux crossing the air-gap, produced by unit *resultant* current encircling the polar axis (direct position).
λ = flux linkage per phase due to flux crossing the air-gap, produced by unit current encircling the inter-polar space (transverse position).

LIST OF SYMBOLS—*continued*

Both κ and λ are measured in volt-seconds.

$x_d = (l + \kappa)\omega$ = total reactance per phase in the direct position.
$x_t = (l + \lambda)\omega$ = total reactance per phase in the transverse position.
σ = the ratio $\frac{x_d}{x_t}$
R = armature resistance per phase.
$Z^2 = (R^2 + x_d x_t)$
$Z_d^2 = (R^2 + x_d^2)$
$Z_t^2 = (R^2 + x_t^2)$
Z_s = synchronous impedance.
M = coefficient of mutual inductance between armature phase and the field winding.
$\rho' = \frac{M\omega}{x_d}$
$\rho = \frac{\rho' x_d x_t}{Z^2}$
p = number of pole-pairs.
$\frac{\omega}{2\pi}$ = frequency of supply.

INTRODUCTION

When the theory of the polyphase alternator is to be discussed, one of three methods of approach is generally used. These may be distinguished as

(*a*) The method of the ampere-turn triangle;
(*b*) The method of the voltage polygon;
(*c*) A combination of (*a*) and (*b*), sometimes called the wattless-current method;

and they may be summarized in the following way.

(a) The Ampere-turn Triangle

Two sources of magnetomotive force are present in the machine, one due to the ampere-turns supplied by the field and the other due to those supplied by the armature. The two acting together must produce a flux sufficient to generate the back-e.m.f. The terminal voltage is obtained by adding or subtracting the armature resistance-drop; thus in Fig. 1, which represents phase conditions for a motor working on terminal voltage OV and taking a leading current OI, if OP is the resistance-drop then this subtracted vectorially from OV gives OE which must be equal and opposite to the back-e.m.f. OE′. If OF represents the ampere-turns necessary to produce the flux generating OE′ and OB the ampere-turns due to the armature current, then the field ampere-turns must be represented by BF.

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* The Papers Committee invite written communications, for consideration with a view to publication, on papers published in the *Journal* without being read at a meeting. Communications (except those from abroad) should reach the Secretary of The Institution not later than one month after publication of the paper to which they relate.

This diagram is the basis of the Rothert method* of predicting regulation in which the armature and field ampere-turns are translated into terms of field currents with the help of the open-circuit and short-circuit curves; this latter referring to conditions of saturation corresponding to the small voltage required to maintain the short-circuit current. When the machine is operating on load voltage the saturation in the main flux path is increased, as is also the magnetic potential across the field system; the armature and field leakages are therefore higher than before, and a given armature current, considered as a generator of flux in the armature coil, becomes, in terms of the field current, larger than it was on short-circuit.† When used for predicting regulation, therefore, the Rothert tests give values for the voltage-drop which are too small for lagging generator currents.

From the point of view of mathematical analysis the ampere-turn triangle is somewhat cumbersome. The voltage equations are

$$v = \frac{d\Phi}{dt} + Ri$$

$$\Phi^2 = c^2(\mathrm{BF}^2 + \mathrm{BO}^2 - 2\mathrm{BO}\,.\,\mathrm{BF}\cos\angle\,\mathrm{OBF})$$

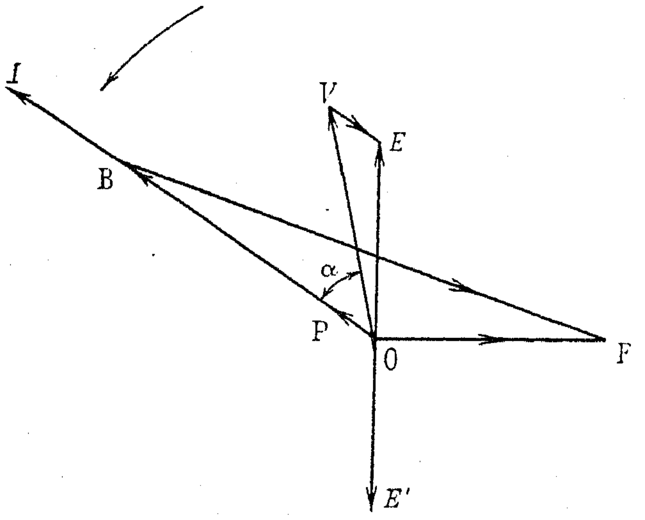

Fig. 1

where v is the terminal voltage, i the armature current, and c a factor converting ampere-turns to fluxes.

(b) The Voltage Polygon

Two sources of e.m.f. are considered as present in the machine, one due to the cutting by the armature coils of the flux which would be produced by the field magnet if acting along, and one due to the inductive effect of the *total* flux which would be produced in the armature coils by the armature current if the field flux were not present. The e.m.f. due to the field flux is now considered as the generated e.m.f. of the machine and the terminal voltage obtained by the addition or subtraction of the resistance drop in phase with the armature current, and the drop due to the armature flux in leading quadrature with it. Thus in Fig. 2, showing phase conditions for a motor, OI is the current leading by an angle α on the terminal voltage OV, OQ is the resistance drop and QP the inductance drop per phase. OP is their resultant.

If EV = OP then the field excitation must be such that the field flux when acting alone would generate an e.m.f. equal and opposite to OE.

This diagram is particularly valuable when the performance of the machine is to be expressed mathematically. For a symmetrical machine* we have at once as the e.m.f. equation,

$$v = \frac{d\Phi}{dt} + L\frac{di}{dt} + Ri$$

where v = applied voltage,
Φ = armature flux linkage due to the field acting alone (in volt-seconds),
L = self-inductance of the armature coil,
R = armature resistance, and
i = armature current.

Difficulty occurs, however, when an attempt is made to estimate the value of L, since, though it appears in the equation as constant, it is in general a function of Φ, i, and v. The assumption is also made that a field current which would produce Φ under open-circuit conditions would produce the same flux linkage when acting alone under the conditions of saturation appropriate to a given load. This, though giving rise in general to smaller errors than the assumption of a constant L, is also objectionable.

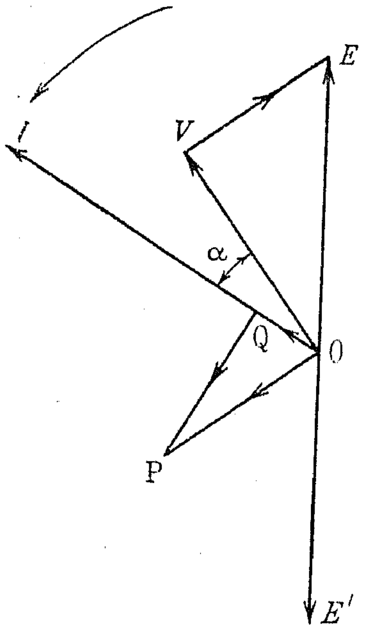

Fig. 2

The diagram is the basis of the Behn–Eschenberg method† of predicting regulation, and here L is assumed to be equal to the synchronous reactance of the machine as determined from the open-circuit and short-circuit curves. This determination of L usually gives a value which is much too high, making the predicted regulation " pessimistic " in its estimate of the voltage-drop for a generator on a lagging-power-factor load. The reason for this is not far to seek, for taking a given armature current I_a which requires a field current I_f to drive it in the short-circuit, the synchronous impedance is defined as $Z_s = V/I_a$ if V is the voltage which I_f would produce on open-circuit. In general V is much smaller than the load voltage of the machine and occurs on the straight portion of the open-circuit curve before saturation has begun, and it is manifestly not permissible to apply the value Z_s so derived when the load voltage is under consideration, if this latter voltage occurs above the knee of the curve. It is perhaps of interest to note that though the Behn–Eschenberg and Rothert predictions of regulation give such widely different results when the machine is working on the saturated part of the open-circuit curve, they both rely on the same experimental data.

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* *Elektrotechnische Zeitschrift*, 1899, vol. 20, p. 619.

† See Fig. 18.

* That is, a machine in which the magnetic reluctance in the path of the polar-axis flux is the same as that in the path of the flux in the cross axis.

† *Electrician*, 1895, vol. 35, p. 424.

(c) The Wattless-current Method

Here the armature leakage flux is differentiated in its effects from the flux crossing the air-gap, which is generally referred to as the flux of armature reaction. The back e.m.f. of the machine is assumed to be due to a flux which links both armature and field and is produced by the resultant ampere-turns of the two; the terminal voltage is then obtained by adding or subtracting the armature resistance-drop (in phase with the current) and the armature *leakage* reactance-drop (in leading quadrature with the current).

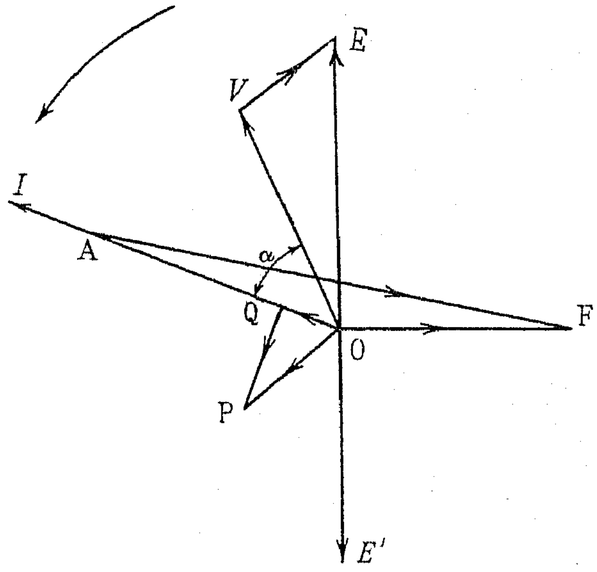

Fig. 3A

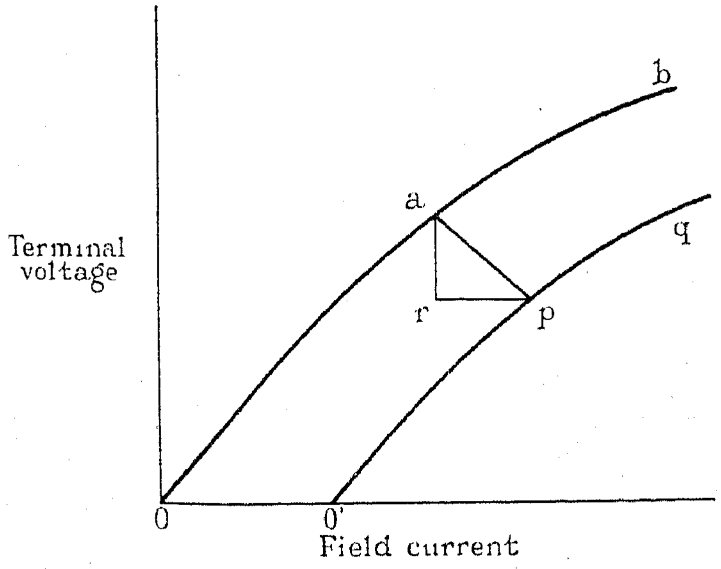

Fig. 3B

Thus in Fig. 3A, which represents phase conditions for a motor taking a leading current OI at a voltage OV. The generated e.m.f. is equal and opposite to OE which is found by subtracting from OV the resistance drop OQ and the *leakage* reactance-drop QP. The generated e.m.f. requires a common flux to produce it, and this in turn requires a number of ampere-turns equal to OF. Then if the armature ampere-turns effective in the production of this flux are equal to OA, the effective field ampere-turns must be AF.

The method derives its name from the tests which are applied when it is to be used for predicting regulation. If an open-circuit curve oab (say) is available for a given machine, and a curve o′pq (say) between voltage and field current on a constant wattless current I_L, then if oab is a similar curve to o′pq it may be regarded as oab displaced downwards by the amount ar, and to the right by the amount rp, if a and p are the corresponding points on the two similar curves* (see Fig. 3B). If the wattless current I_L is equal to the armature current OI in Fig. 3A, then " ar " is assumed to be equal to QP and " rp " to be the field equivalent of the armature demagnetizing ampere-turns due to the current OI.

Reviewing the three methods, it may be said, first, that in each of them the machine under consideration is tacitly assumed to behave as if it were symmetrical. As shown later, this need not introduce* a large error in the prediction of regulation but may seriously affect the calculation of the load angle for the asymmetrical case.† Secondly, they assume that an irregular wave of flux, current, or e.m.f., can be replaced, for the purposes of vector representation, by an equivalent sine wave; and thirdly, they take no account of the wave-shape of the current in the busbars to which the machine may be connected.

The foregoing methods were evolved at a time when it was difficult to take large lagging currents from a machine, except on short-circuit. At the present time, however, several large test-beds have synchronous machines available which can absorb wattless currents considerably larger than the full-load currents of the machines under test. This being so, it is useful to combine the previous methods, since by doing so we can obtain a vector diagram and method of measurement which are to be preferred on the score of accuracy.

The authors' method will be considered for four cases, namely for an asymmetrical machine on infinite busbars,‡ and on busbars of varying voltage; and subsequently for a symmetrical machine on infinite and variable bars, armature resistance being neglected in the first approximation.

Development of the Method

Attention is concentrated on the conductors of one phase of the armature, to the terminals of which a voltage V is applied; this presupposes a back-e.m.f. equal and opposite to V which must in turn be generated by a rate of change in flux linkage $d\Phi/dt$ in the armature. If V is constant, $d\Phi/dt$ must be constant also, and since the speed is synchronous we have, assuming sine functions,

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* See Potier: *Éclairage Electrique*, 1900, vol. 35, p. 133.

* See case (2*a*), page 128.

† A machine in which the reluctance in the path of the axial flux differs from that in the path of the cross flux, e.g. a salient-pole machine.

‡ Busbars whose voltage and frequency are invariable.

$$V = \Phi\omega$$

where V and Φ are respectively the root-mean-square values of the phase voltage and flux linkage, and $\omega = 2\pi \times$ (frequency of supply). Thus any voltage may be replaced by a corresponding flux linkage. This linkage is produced by a flux which may be generated by the field, or by the armature, or by the resultant ampere-turns of the two acting together. If the armature is carrying current, the flux will be partly leakage flux and partly flux which links both armature and field. In cases of steady operation this distinction need not trouble us, though when phase-swinging occurs it becomes important.*

Case (1). *Asymmetrical Machine on Infinite Busbars.*

In Fig. 4 let OV be the applied phase voltage and OF the flux linkage which its presence requires.

Let FB represent the line of the polar axis. Then the linkage OF may be considered as the vector sum or the two linkages OB and BF. Let OI represent the armature current I leading on OV by the angle α. The angle BFO is the load angle† θ (say).

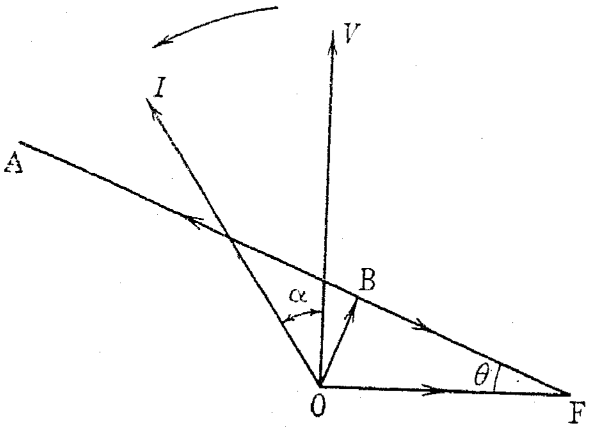

Fig. 4

The current I can now be resolved in the two directions OB and BF. The component of OI in phase with OB is

$$I_1 = I\cos(\theta + \alpha)$$

and I_1 must produce the linkages OB.

Thus $$\mathrm{OB} = I_1(l + \lambda)$$

where l is the flux linkage per ampere, in volt-seconds, due to the leakage flux, and λ the flux linkage due to flux crossing the air-gap, which would be produced by unit armature current encircling the interpolar, or cross, axis.

The armature current in phase with FB is similarly

$$I_2 = I\sin(\theta + \alpha)$$

Now the flux linkage BF is produced by the *resultant* axial ampere-turns of field and armature and may therefore be written

$$\mathrm{BF} = (I_{fa} - I_2)(l + \kappa)$$

where κ is the flux linkage due to flux* crossing the air-gap, which would be produced by unit *resultant* current encircling the polar, or direct, axis, and I_{fa} is the armature equivalent of the field current with which the machine is working. Thus if $\mathrm{AF} = (l + \kappa)I_{fa}$ then $\mathrm{BA} = (l + \kappa)I_2$ and $\mathrm{OB} = (l + \lambda)I_1$.

If I_0 is the armature equivalent of the field current required to give the phase voltage V on open-circuit, then $$\mathrm{OF} = (l + \kappa)I_0 \text{ (if } \theta \text{ is small).}$$

Dividing all quantities by $(l + \kappa)$, we can draw the current diagram Fig. 5, in which PI_2 is made equal to $\mathrm{BA}/(l + \kappa)$ and is therefore equal to the armature current encircling the axis. OP is made equal to $\mathrm{OB}/(l + \kappa)$ and I_2I_0 to $\mathrm{AF}/(l + \kappa)$. The vector OI_1 is made equal to $\mathrm{OP}(l + \kappa)/(l + \lambda)$; thus OI_1 represents the armature current encircling the interpolar space. The vector I_2I_0 is the armature equivalent of the applied field current, and OI_0 the armature equivalent of the field current for V volts per phase on open-circuit.

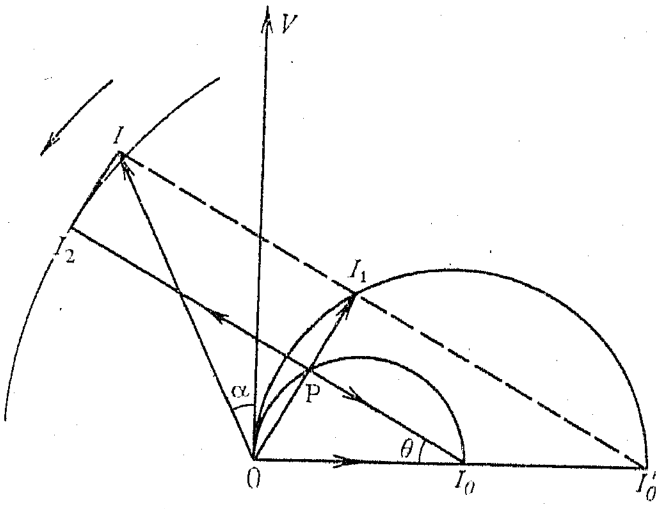

Fig. 5

The angle OPI_0 is a right angle, thus the locus of P is a circle, diameter OI_0. It follows that the locus of I_1 is also a circle, diameter OI_0', where

$$\mathrm{OI}_0'/\mathrm{OI}_0 = (l + \kappa)/(l + \lambda)$$

The total armature current OI is found by compounding OI_1 and PI_2. The locus of I for constant excitation is, as remarked by Ingram, a limaçon.† If OI is above the horizontal line through O the machine is acting as a motor; if below, as a generator.

The armature equivalent of a given field current may be considered as nearly constant at constant terminal voltage if the load angle is so small as to make $\cos\theta$ nearly equal to unity. The value of $(l + \kappa)$ is associated with a certain flux linkage OF (Fig. 4) which is nearly equal to BF if θ is small, and the factor converting field current to equivalent armature current must also be considered in relation to these two linkages. If a field current I_f is applied to the machine, the armature linkage produced by this will be MI_f, where M is the coefficient of mutual induction between field and armature for the

* J. C. Prescott and J. E. Richardson: *Journal I.E.E.*, 1934, vol. 75, p. 501.

† Defined as the electrical angle between the pole centre-line and the vector of resultant flux linkage.

* Cf. J. C. Prescott and E. W. Connon: "Measurement of the Load Angle of Synchronous Machines," *Journal I.E.E.*, 1931, vol. 69, p. 281. Here κ was taken as the flux linkage per phase due to flux crossing the air-gap produced by unit armature encircling the polar axis.

† W. H. Ingram: "The Operability of a Salient-Pole Alternator on an Infinite Bus," *Philosophical Magazine*, 1933, vol. 17, p. 271. Also see Appendix.

conditions of saturation corresponding to the linkage OF (or BF). The armature linkage for a given current I_2 encircling the polar axis under the same condition of saturation would be $I_2(l + \kappa)$; if these are equal then $I_2 = I_f(M/(l + \kappa) : I_2$ being the armature equivalent of the field current I_f. Putting $M/(l + \kappa) = \rho'$ we have in Fig. 4, since $I_{f_a} = \rho' I_f$ and $I_0 = \rho' I_{f_0}$,

$$\mathrm{BF} = (I_{fa} - I_2)(l + \kappa) = (\rho I_f - I_2)(l + \kappa)$$

and $$\mathrm{OF} = I_0(l + \kappa) = \rho' I_{f_0}(l + \kappa)$$

where I_f is the field current with which the machine is operating, and I_{f_0} is the field current required to produce an e.m.f. equal to V on open-circuit.

The diagram is easy to handle, and the following results are readily obtained.* The synchronizing torque, i.e. the torque for unit mechanical angular displacement of the rotating system, is

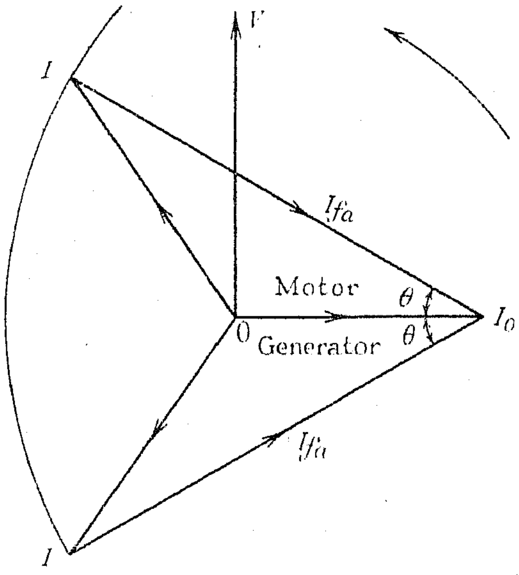

Fig. 6

$$\mathbf{T}_s = \frac{V(I_{fa}\cos\theta + I_0(\sigma - 1)\cos 2\theta)p^2}{9\cdot 81\omega}\ \text{kg.-m. per phase}$$

where σ represents the ratio $(l + \kappa)/(l + \lambda)$, I_{fa} is the armature equivalent of the field current with which the machine is working, and p is the number of pole-pairs.

The load angle for maximum output is given by

$$\theta_{max.} = \text{arc}\cos\left[-\frac{K}{4} + \sqrt{\left(\frac{K^2}{16} + \frac{1}{2}\right)}\right]$$

where $$K = \frac{I_{fa}}{I_0(\sigma - 1)} : \sigma > 1$$

Putting $I_{fa} = 0$ in the expression for $\theta_{max.}$, we have $\theta'_{max.} = 45°$,† and thus the maximum load which the machine will take with an unexcited field is

$$W_{max.} = VI_0\left(\frac{\sigma - 1}{2}\right)\ \text{watts per phase.}$$

Certain precautions must, however, be observed in assessing the magnitude of the vectors. These will be discussed more fully under "Measurements"; for the present it is sufficient to point out that if θ is large the resultant flux along the axis BF may differ considerably from that along OF corresponding to the terminal voltage. The saturation of the iron will not then be the same along AF and OF, and the flux linkage per ampere, $(l + \kappa)$, will not be a constant. With load angles up to 10° or 15°, however, the error introduced by considering $(l + \kappa)$ as constant is only small. In the second place, it cannot be too strongly emphasized that σ, I_0, and I_{f_a}, are assumed to be known for the identical conditions of field current, terminal voltage, load angle, and current, along and across the axis, under which the machine is operating.

Case 1(*a*). *Symmetrical Machine on Infinite Busbars.*

This case may be treated by making σ equal to unity in Case 1. The diagram is shown in Fig. 6, the vectors having the same significance as their counterparts in Fig. 5.

The locus of I for constant excitation is now a circle with centre I_0 and radius I_{fa}. The synchronizing torque is

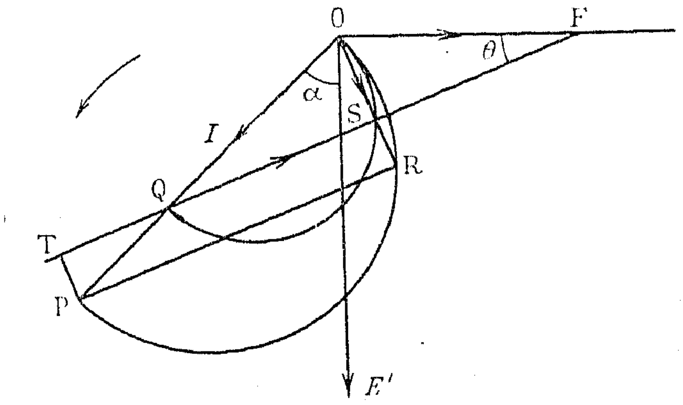

Fig. 7

$$\mathbf{T}_s = \frac{V(I_{fa}\cos\theta)p^2}{9\cdot 81\omega}\ \text{kg.-m. per phase}$$

and the load angle for maximum output at a given field is $\frac{1}{2}\pi$.

It is seen that a symmetrical machine gives a larger angular deflection for a given load and is less "stiff" than one which is asymmetrical, but can be deflected through a larger angle without becoming unstable. Other things being equal, a barrel-rotor machine will have a longer periodic time than a machine with salient poles.

Case (2). *Unsymmetrical Generator on Busbars of Variable Voltage.*

Let the current again be OI at a power factor angle α. Let OF in Fig. 7 be the resultant flux linkage which will sustain a terminal voltage V.

On OI describe a circle diameter $\mathrm{OP} = (l + \kappa)I$ and a circle diameter $\mathrm{OQ} = \mathrm{OP}(l + \lambda)/(l + \kappa)$. Consider any point R on the former, and let RO cut the latter at S. We then have

$$\mathrm{PR} = (l + \kappa)I\sin(\theta + \alpha)$$
$$\mathrm{RO} = (l + \kappa)I\cos(\theta + \alpha)$$
$$\mathrm{SO} = \mathrm{RO}\frac{(l + \lambda)}{(l + \kappa)}$$
$$\mathrm{OE}' = -V$$

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* See Appendix.

† Cf. C. P. Steinmetz: "Theory and Calculations of Electrical Apparatus," p. 270, 1917 edn.

Let FS produced meet the perpendicular from P at T, then if FT is the flux linkage produced by the field the vector sum of FT, OS, and TS, must be the resultant flux linkage OF. Remembering that $OF = (l + \kappa)I_0$, where I_0 is the armature current which, when acting alone, would produce the linkage OF, and that $TF = (l + \kappa)I_{fa}$, where I_{fa} is the armature equivalent of the field current required for the production of TF, we may divide all the vectors by $(l + \kappa)$ and obtain the current diagram Fig. 8, in which

OI = armature current

TI_0 = armature equivalent of the applied field current $= \rho' I_f$

OI_0 = armature equivalent of the field current corresponding to the working voltage $= \rho' I_{f_0}$

$RI = I_2$

$OR = I_1$

$OS = \dfrac{I_1}{\sigma}$

$OE' = -V$

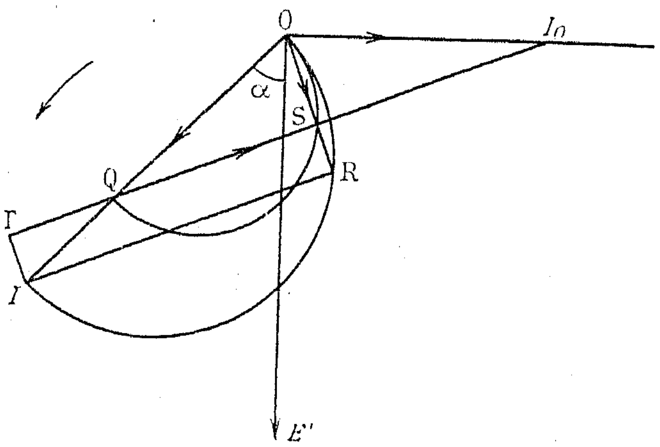

Fig. 8

In order that the diagram may be applied to the predetermination of regulation, the saturation curve of the machine must be available and the values of $(l + \lambda)$ and ρ' known for all conditions of saturation under which the machine is to operate. If this information is accessible, a terminal voltage V_1 may be assumed and the field current for a given armature current at a given power factor calculated in the following way.

From the saturation curve (Fig. 9) we have:—

Field current for voltage $V_1 = I_{f_1}$

Equivalent armature current for voltage $V_1 = I_{f_1}\rho'$

$(l + \kappa)\omega = \dfrac{V}{I_{f_1}\rho'}$

$(l + \lambda)\omega$ is known and thus σ is defined.

Set off the current vector OI (Fig. 8) at the power factor angle α. Describe a circle on OI as diameter and a second circle tangential to the first at O of diameter $(OI)/\sigma$. Let this cut OI again in Q. Set off $OI_0 = I_{f_1}\rho'$. Join I_0Q and produce; drop the perpendicular IT from I on to I_0Q produced, then TI_0 is the armature equivalent of the field current I_{f_2} which must be applied to the machine, and $I_{f_2} = TI_0/\rho'$. From the saturation curve the corresponding voltage V_2 is found and hence the regulation (up) $= (V_2 - V_1)/V_1$.

Case (2a). Symmetrical Machine on Busbars of Variable Voltage.

Writing, as before, $\sigma = 1$ gives Fig. 6, from which the regulation can be determined as it was in Case (2), I_0I now being the armature equivalent of the field current which must be applied to the machine.

It is interesting to notice that, particularly on lagging power factors, the asymmetry of a machine makes little difference to its regulation, for, referring to Fig. 8, it is seen that, while the equivalent field for the salient-pole machine is TI_0, a symmetrical machine would require II_0. This conforms with the general experience that machines in which the ratio (pole arc)/(pole pitch) is large have the regulation properties of barrel-rotor machines.

From the point of view of the applicability of the diagram this is fortunate, since it is not necessary to know the value of $(l + \lambda)$ very accurately.

Fig. 8 is in effect the Rothert regulation diagram expressed in terms of armature instead of field currents. By applying the diagram in the manner described in this paper, however, it is made to give more accurate results than are generally associated with the Rothert method.*

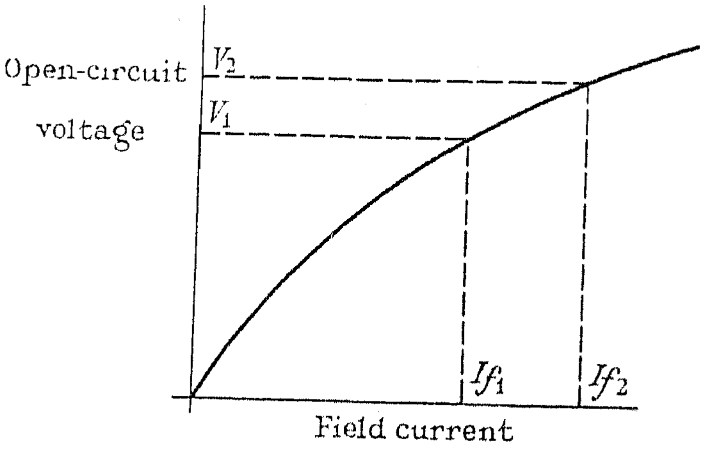

Fig. 9

The Effect of Armature Resistance

Case (1). Unsymmetrical Machine on Infinite Busbars.

If a current i flows in a conductor of resistance R, a voltage Ri in phase with i is required to maintain it. If this voltage is expressed in terms of a changing flux linkage, then if $i = I \sin \omega t$ the voltage is $RI \sin \omega t$ and the flux linkage $\Phi'_R = -\int RI \sin \omega t \, dt$. Thus we may say that the current i flowing in the resistance R produces, in effect, a flux equal and opposite to Φ'_R; in other words, this flux linkage must be obliterated before the current can flow. If this flux be called Φ_R then $\Phi_R = -(RI/\omega) \cos \omega t$. Vectorially, if i is drawn vertically upwards Φ_R is to be drawn horizontally to the right. Fig. 4 may now be re-drawn to include armature resistance. As before, $OF = V/\omega$, but for a motor $BF = (I_{fa} - I_2)(l + \kappa) + (RI_1/\omega)$ and $OB = I_1(l + \lambda) + (RI_2/\omega)$.† Writing for simplicity $(l + \kappa)\omega = x_d$ and $(l + \lambda)\omega = x_t$ the following equations must be satisfied, since ∠ OBF is still a right angle and OF still equal to $\rho' I_{f_0}(l + \kappa)$:—

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* See "Measurements."

† The generator case may be treated by writing $-R$ for R in the motor case.

$$\rho' I_{f_0} x_d \cos\theta = (\rho' I_{f_a} - I_2) x_d + R I_1 \quad . \quad . \quad (1)$$

$$\rho' I_{f_0} x_d \sin\theta = I_1 x_t + R I_2 \quad . \quad . \quad . \quad . \quad (2)$$

where $\rho' I_f$ is the armature equivalent of the field current with which the machine is operating, and $\rho' I_{f_0}$ the armature equivalent of the field current required to produce an e.m.f. equal to OV on open-circuit.
Hence

$$I_1 = \frac{\sigma x_d x_t(\rho' I_{f_0} \sin\theta) - R x_d \rho'(I_f - I_{f_0}\cos\theta)}{Z^2}$$

$$I_2 = \frac{x_d x_t \rho'(I_f - I_{f_0}\cos\theta) + R x_d \rho'(I_{f_0}\sin\theta)}{Z^2}$$

where $Z^2 = (R^2 + x_d x_t)$, I_f is the field current applied, and I_{f_0} is the field current corresponding to OV on open circuit.

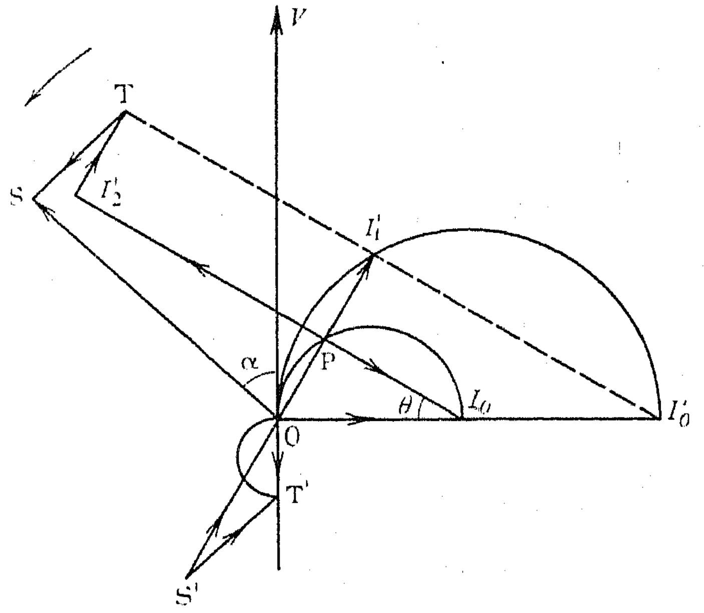

Fig. 10

If θ is zero,

$$I_2 = \frac{\rho x_d x_t}{Z^2}(I_f - I_{f_0})$$

$$\frac{I_2}{I_f - I_{f_0}} = \rho' \frac{x_d x_t}{Z^2} = \rho \text{ (say)}$$

Thus I_2 is now the armature equivalent of the field current $(I_f - I_{f_0})$ and in expressing the components of the armature current in terms of field current ρ must now be used instead of ρ'. Thus

$$I_1 = \rho\left[\sigma I_{f_0}\sin\theta - \frac{R}{x_t}(I_f - I_{f_0}\cos\theta)\right]$$

$$I_2 = \rho\left[(I_f - I_{f_0}\cos\theta) + \frac{R}{x_t}(I_{f_0}\sin\theta)\right]$$

or if I_0 is now written $I_0 = \rho I_{f_0}$ (instead of $I_0 = \rho' I_{f_0}$) and I_{f_a} written $I_{f_a} = \rho I_f$ (instead of $I_{f_a} = \rho' I_f$) we have for a machine in which there is armature resistance

$$I_1 = \sigma I_0 \sin\theta - \frac{R}{x_t}(I_{f_a} - I_0\cos\theta) \quad . \quad . \quad (3)$$

$$I_2 = (I_f - I_0\cos\theta) + \frac{R}{x_t}(I_0\sin\theta) \quad . \quad . \quad (4)$$

The first terms on the right of the expressions for I_1 and I_2 give the same diagram as Fig. 5 if the new significance is given to I_0 and I_{f_a}. The second terms give a diagram similar to I_2I_0O, but reduced in the ratio of $R : x_t$ inverted and turned through a right angle. The complete diagram for a motor is thus Fig. 10, in which:—

$$OI_0 = I_0 = \rho I_{f_0}$$

$$OI_0' = \sigma I_0 : \sigma = \frac{x_d}{x_t}$$

$$I_0I_2' = \rho I_f = I_{f_a}$$

$$OT' = \frac{R}{x_t}OI_0 = \frac{R}{x_t}I_0$$

$$OS' = \frac{R}{x_t}I_0I_2' = \frac{R}{x_t}I_{f_a}$$

$$TS = -\,T'S'$$

The resultant current is the vector sum of OI_1', PI_2', and TS. It is shown at OS. The load angle is θ. The application of the diagram is simple; for instance, the power component of the current may be written at once as

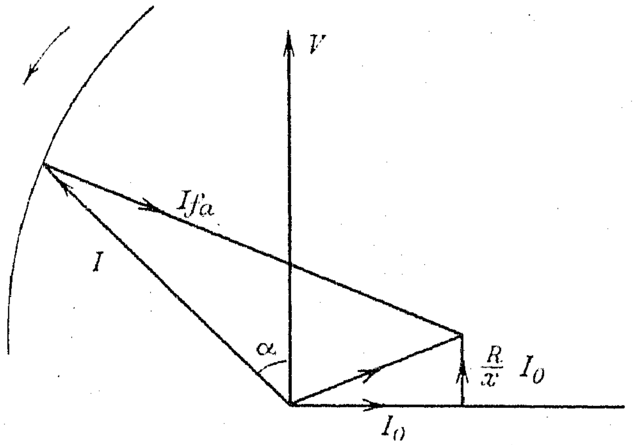

Fig. 11

$I_W = I_1\cos\theta + I_2\sin\theta$, which, from (3) and (4), gives

$$I_W = I_{f_a}\sin\theta + \left(\frac{\sigma - 1}{2}\right)\sin 2\theta - \frac{R}{x_t}(I_{f_a}\cos\theta - I_0)$$

from which the relation between load angle and load may be plotted.* The locus of the extremity of the vector of armature current is again a limaçon.† Motor action is implied when the current vector is above the X axis; generator action when it is below.

It is to be noted that as the point P moves over the circle on diameter OI_0, to include both motor and generator action, the vector OT′ is always drawn vertically downwards, and thus when $I_2'I_0$ is above the X axis S′O is in phase with OP, and when $I_2'I_0$ is below the axis S′O is in antiphase to OP.

Case 1(*a*). *Symmetrical Machine on Infinite Busbars.*

Putting $\sigma = 1$, the circle of diameter OI_0' in Fig. 10 disappears. TS is always at right angles to I_0I_2 and always proportional to it. Thus, as shown by Wall,‡ the locus of the extremity of the current vector is a circle of radius $I_{f_0}(Z/x)$ whose centre is at the point $\left[I_0; \left(\frac{R}{x}\right)I_0\right]$, where $x_d = x_t = x$, and $Z^2 = (R^2 + x^2)$ (see Fig. 11).

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* See "Experimental Work."

† See Appendix.

‡ T. F. Wall: "Theory of the Three-Phase Synchronous Machine," *Journal I.E.E.*, 1914, vol. 52, p. 277.

Cases (2) *and* (2*a*). *Machine on Bars of Variable Voltage.*

Since, in treating problems of regulation, the armature current and its power factor may generally be assumed as the starting point, it is best to use the simpler diagrams (Figs. 6 and 8) for the calculation of the field current required for a given terminal voltage without resistance, and to correct subsequently for resistance by subtracting the armature drop at its appropriate phase angle. It should be pointed out that if the simple diagram is used, then the factor ρ' must be taken instead of ρ (though these will differ by less than 1 % in machines of normal design).

Measurement.

In order that the diagrams may be applied, the magnitude of three quantities σ, ρ, and R, must be known and the saturation curve for the machine must be available. Fortunately, it is possible to obtain the relevant information with sufficient accuracy without applying load to the machine.

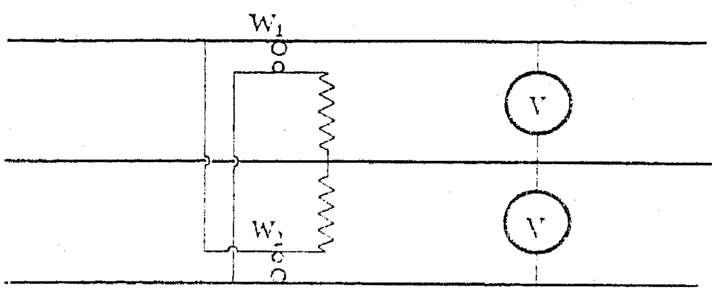

Fig. 12

The Value of ρ.

If the machine is connected to infinite bars of voltage V and operated on no load, then the angle θ is small and in equation (4) $\cos\theta \simeq 1$, while the term in $R/(x_t)$ may be neglected if R is small compared with x_t; thus

$$\frac{I_2}{I_f - I_{f_0}} = \rho$$

Starting with $I_2 = 0$, that is with the field current equal to that required for a voltage V on open circuit, if the field current is increased to I_f and the change in I_2 noted the ratio ρ is defined. It is sometimes difficult to reduce the load angle of the machine to zero, owing to the presence of friction, windage, etc., but since the current I_2 is nearly equal to the wattless current supplied to the machine, cross-connected wattmeters may conveniently be used as shown in Fig. 12. Then if W_1 and W_2 are the wattmeter readings (which should be equal), and V is the line voltage, the current in quadrature with V will be

$$I_L = \frac{W_1 + W_2}{2V}$$

and since $I_2 \simeq I_L$ the ratio ρ becomes $\rho = I_L/(I_f - I_{f_0})$ for small values of θ.

In carrying out tests to determine the value of ρ it was found to vary with I_0 and with I_L. The former variation was noticed in all the machines which were tested, while the variation with I_L was pronounced in one only, though there is generally a difference between the values for positive and negative I_L. If the performance of the machine is to be specified completely, ρ should be determined for all values of I_L and I_0 (i.e. terminal voltage) with which it is likely to operate.

The Value of σ.

As has been mentioned before, σ is the ratio x_d/x_t, and by definition $x_d = V/(\rho' I_{f_0})$, where V is the open-circuit voltage for a field current I_{f_0}. The method of estimating the value of ρ has been described in the previous paragraph, and since the value of ρ' differs from that of ρ only by a small amount (it is shown later that this is in general less than 1 %) ρ may replace ρ' and we may write $x_d = V/(\rho I_{f_0})$. It remains then to estimate the value of x_t. If the direct-current field is removed synchronous operation will be maintained on no load if the machine is asymmetrical, and will continue even with a small reversed field. If the reversed field is increased, however, the machine will ultimately fall out of step and slip back through one pole-pitch, after which synchronous operation will be resumed. During the pole-slipping period the field system occupies at one instant a position with respect to the armature field such that all the flux corresponding to the terminal voltage is produced in a direction across the polar axis ($\theta = \frac{1}{2}\pi$), while the flux due to the reversed field is obliterated by a current surrounding the polar axis (i.e. for $\theta = \frac{1}{2}\pi$ in phase with V). The maximum reading of cross-connected wattmeters measures the current in phase with the resultant flux, i.e. in quadrature with V at this instant. If this current be I'_L, then

$$x_t = \frac{V + \rho I'_f R}{I'_L}$$

where V is the terminal voltage and I'_f the reversed field.

If the machine is fitted with damping grids, the calculated value of x_t may be affected by the circulating currents in these during the transit over the pole-pitch. In order to minimize the error thus introduced, it is important that the transit should take place as slowly as possible. This may be ensured by arranging that the reverse field applied is only just sufficient to cause the machine to fall out of step.

GENERAL OBSERVATIONS ON THE AUTHORS' METHOD

The full application of the method presupposes the accessibility of busbars whose voltage can be varied throughout the whole operating voltage range of the machine under test and which shall be capable of giving a wattless current possibly as much as twice the steady short-circuit current of the machine. This is considered to be the most serious drawback to the method.

Something may, however, be added on the credit side.

The values of x_d and ρ are determined by measurement for the identical conditions of terminal voltage, field excitation, and wattless current, under which the machine will operate on load. Thus allowance is made automatically for the effects of saturation in the axial flux path, except in so far as they may be modified by the cross-axis flux. That this is so may be seen from the following considerations. In the determination of ρ the machine is connected unloaded to busbars of voltage V (say), and the field current I_f adjusted till the wattless armature current is zero. Let this value be I_{f_0}. If an increase of δI_f now causes a wattless armature current I_2 to flow, while V remains constant, then

$$\rho = \frac{I_2}{\delta I_f}$$

and $I_2 = \rho\delta I_f$ is the armature equivalent of the field current at terminal voltage V and wattless current I_2.

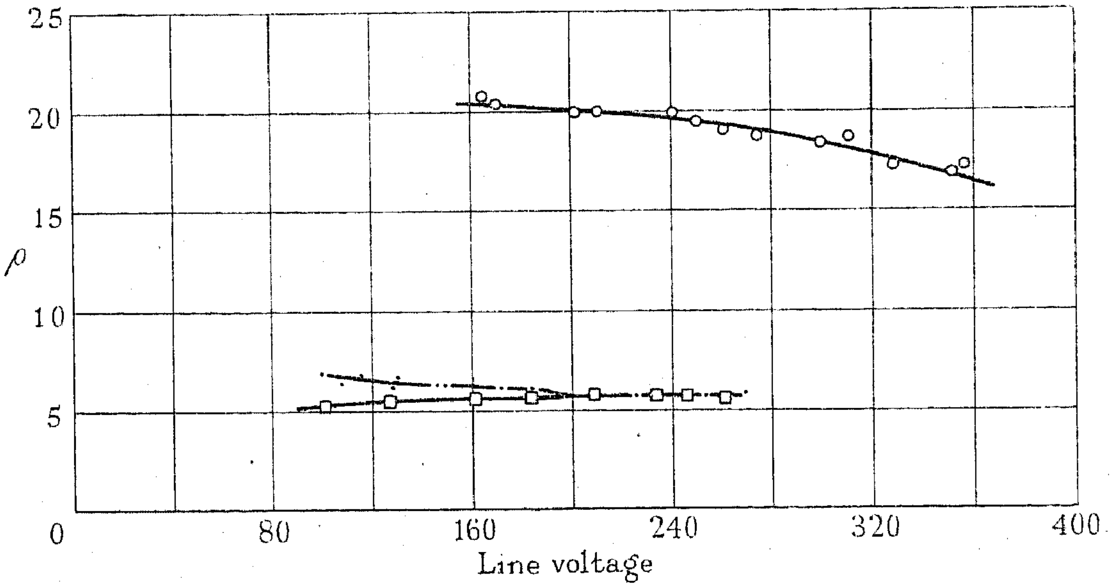

Fig. 13

—○— Machine 1. All currents, magnetizing and demagnetizing.
—·— Machine 3. Magnetizing } all currents.
—□— Machine 3. Demagnetizing } all currents.

Similarly ρI_{f_0} is the armature equivalent of the field current necessary to generate an e.m.f. equal and opposite to the voltage of the busbars under the conditions of saturation implied by the value of the voltage V.

By definition, $x_d = V/(\rho' I_{f_0})$, but without introducing any serious error we may write $x_d = V/(\rho I_{f_0})$, since $\rho = x_d x_t \rho'/Z^2$, and even in a small machine, where the resistance to inductance ratio is comparatively large, the ratio ρ/ρ' is very nearly unity.

For instance, taking a case where $R = 0{\cdot}14$ ohm, $x_d = 2$ ohms, $x_t = 1$ ohm

$$\frac{\rho}{\rho'} = \frac{2}{0{\cdot}02 + 2} = 0{\cdot}99$$

The estimation of the cross-axis reactance x_t is not equally rigorous. For the predetermination of regulation it is not necessary that it should be so, though for calculation of load angle an exact estimation of x_t is important.

The measured value of the quantity is associated with a certain terminal voltage (which should be as near as possible to the working voltage), a certain field current, and a certain wattless armature current. The field current in question is the (small) reverse field which becomes a direct field after pole-slipping has occurred, and the armature current is the maximum wattless current which the machine takes in its transit between poles and which, at the instant of maximum, encircles the interpolar space.

It has been shown in a previous paper* that if the voltage polygon (Method 2 of the Introduction) is taken as the basis of the vector diagram, then x_t decreases more or less rapidly with increase of excitation, and measurements have also shown that, when used in this polygon, x_t must also be considered as a function of the cross-axis current, decreasing rapidly at first and then more slowly as the current increases.

The same limitations do not appear if the ampere-turn triangle is applied in the manner suggested in the present paper, and only at high field currents is there any considerable discrepancy† between calculated and observed values of the load angle. When the voltage polygon is used, both x_d and x_t must be assumed to vary with field current, armature current, power factor, and terminal voltage; when the present method is applied, however, it is in general sufficiently accurate at a given terminal voltage to consider x_t as a constant, measured in the way that has been described, while a value of ρ, and hence of x_d, is associated with each value of the current encircling the polar axis.

With regard to harmonics in current and voltage waves, it must be said at once that the vector diagrams only represent the working conditions so far as the fundamental frequency is concerned. The same limitation does not apply to the measured values of ρ, x_d, and x_t, which may be considered as "effective" values since they take account of the harmonics actually present in the wattless current and terminal voltage. The diagrams in which these quantities appear may therefore be taken to represent conditions for the effective sine waves which have been assumed to replace the distorted waves actually present—a device frequently used in the design of machines.

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* Prescott and Connon, *loc. cit.*

† See Fig. 16B (generator with 6-amp. field), the largest discrepancy observed.

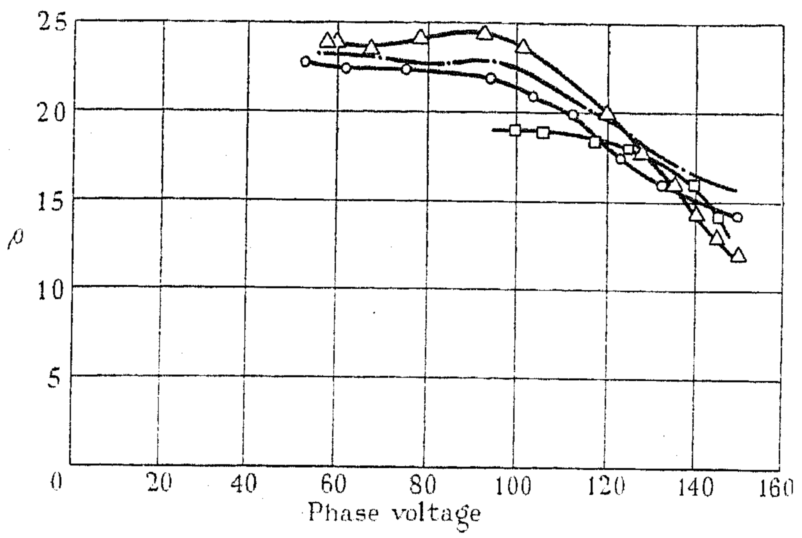

Fig. 14.—Machine 2.

—△— 20 amps. }
— - — 30 amps. } demagnetizing.
—○— 40 amps. }
—□— All currents, magnetizing.

Strictly speaking, of course, the measured values only apply to the machine in operation on the busbars with which the test is made, and an alteration in the waveform of the bars may cause an alteration in the measured values.

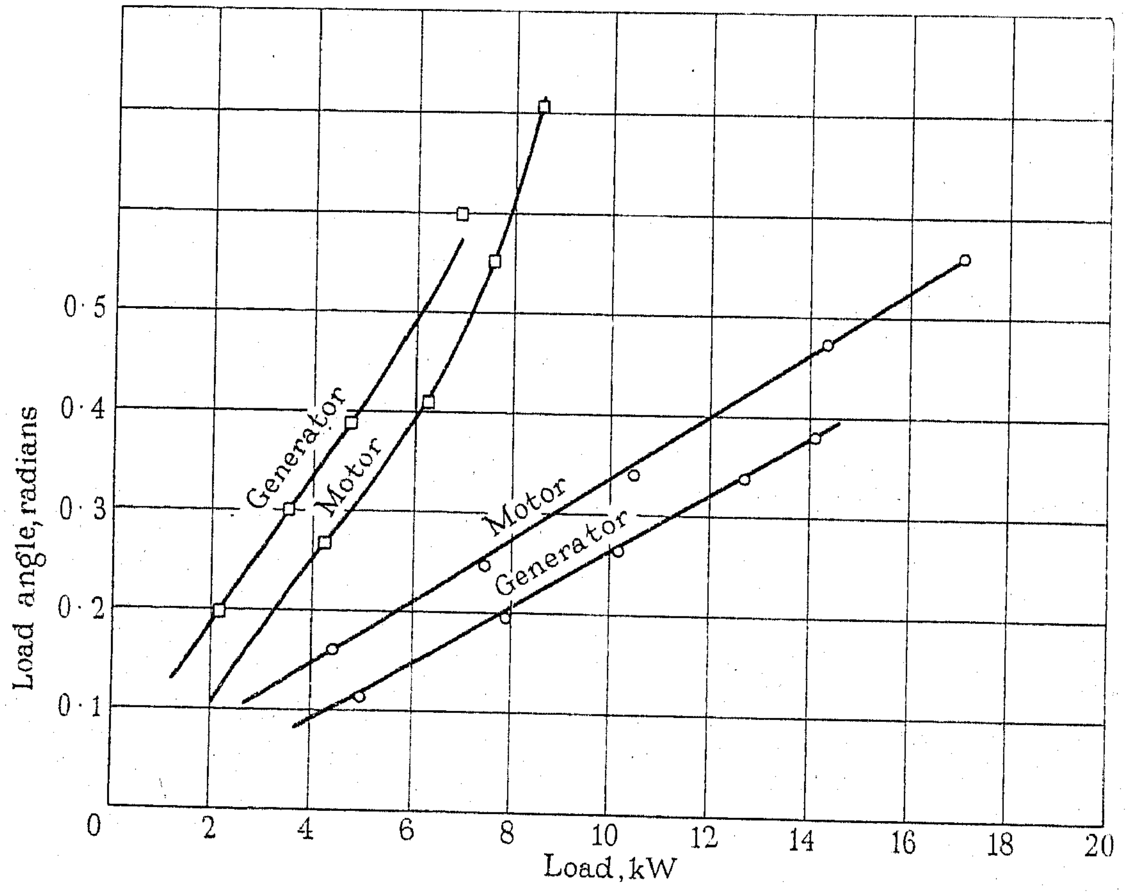

Fig. 15.—Machine 1.

—○— Field 3 amps.
—□— Field 0·5 amp.

EXPERIMENTAL WORK

A series of experiments was carried out in the Laboratories of Applied Electricity of the University of Liverpool with the object of investigating the applicability of the theory developed in the first part of the paper. A summary of these is reproduced for three machines, the particulars of which are given below.

Machine 1.

Three-phase, star-connected; normal rating 200 volts, 50 amps., 50 cycles; 4 poles; ratio (pole arc/pole pitch) = 0·67; armature resistance = 0·16 ohm per phase; field current for 200 volts on open-circuit = 0·975 amps.; maximum lagging current as machine slips a pole under a reversed field of 1 amp. = 56 amps.

The value of ρ plotted against terminal voltage is given in Fig. 13. In this machine ρ is scarcely affected by the value of I_L.

From the curve, ρ for 200 volts = 20.

Hence

$$\left.\begin{aligned} I_0 &= \rho I_{f_0} = 20(0\cdot975) = 19\cdot5 \text{ amps.} \\ x_d &= \frac{200}{19\cdot5\sqrt{3}} = 5\cdot93. \\ x_t &= \frac{\dfrac{200}{\sqrt{3}} + 20 \times 0\cdot16}{56} = 2\cdot1. \end{aligned}\right\} \text{At 200 volts}$$

$$\sigma = 2\cdot82.$$

Machine 2.

Three-phase, star-connected; normal rating 200 volts, 58 amps., 50 cycles; 6 poles; ratio (pole arc/pole pitch) = 0·583; armature resistance = 0·11 ohm per phase; field current for 200 volts on open-circuit = 3·5 amps.; Maximum lagging current, corrected to 200 volts, as machine slips a pole under a reversed field of 0·75 amp. = 151 amps.

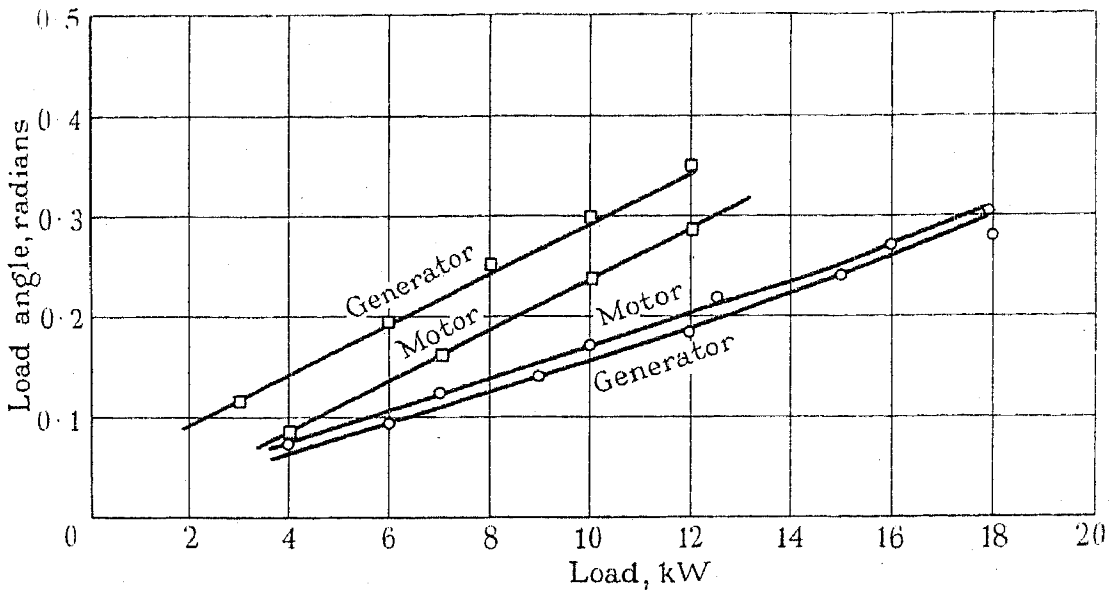

Fig. 16A.—Machine 2.
—○— Field 4·4 amps.
—□— Field 2 amps.

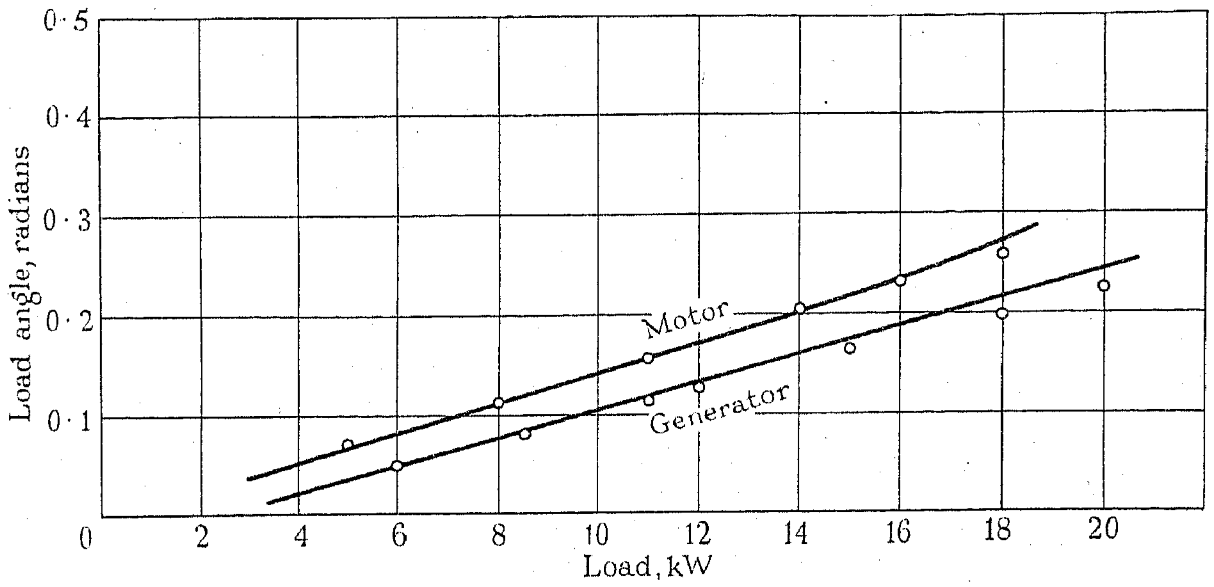

Fig. 16B.—Machine 2, field 6 amps.

The value of ρ plotted against voltage is shown in Fig. 14. In this machine ρ is affected both by V and I_L. At 200 volts the following values apply:—

Lagging current (amps.)	20	30	40
ρ	21·2	20·5	19·5
x_d	1·55	1·6	1·685
x_t	0·77	0·77	0·77
σ	2·01	2·08	2·19

Machine 3.

Three-phase, star-connected; normal rating 200 volts, 14 amps., 50 cycles; ratio (pole arc/pole pitch) $\simeq 1$; armature resistance = 0·27 ohm per phase; field current for 200 volts on open-circuit = 3·1 amps.; maximum

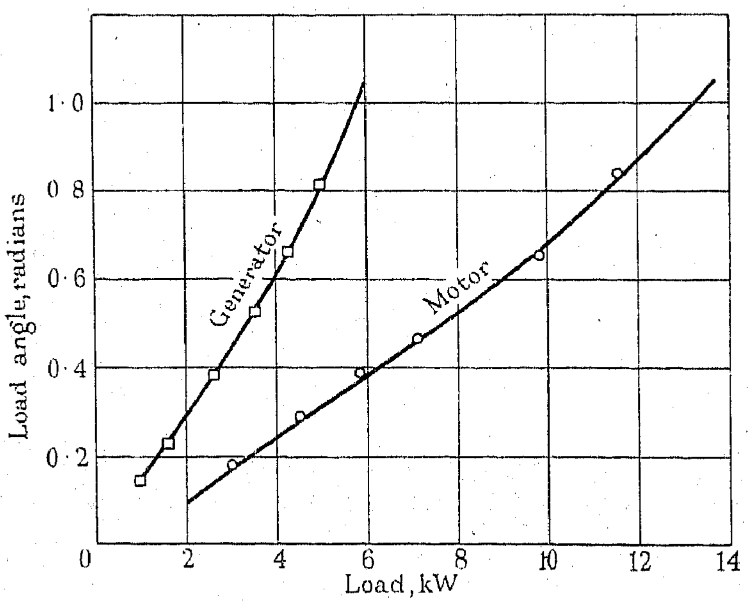

Fig. 17.—Machine 3.
—○— Field 7 amps.
—□— Field 3 amps.

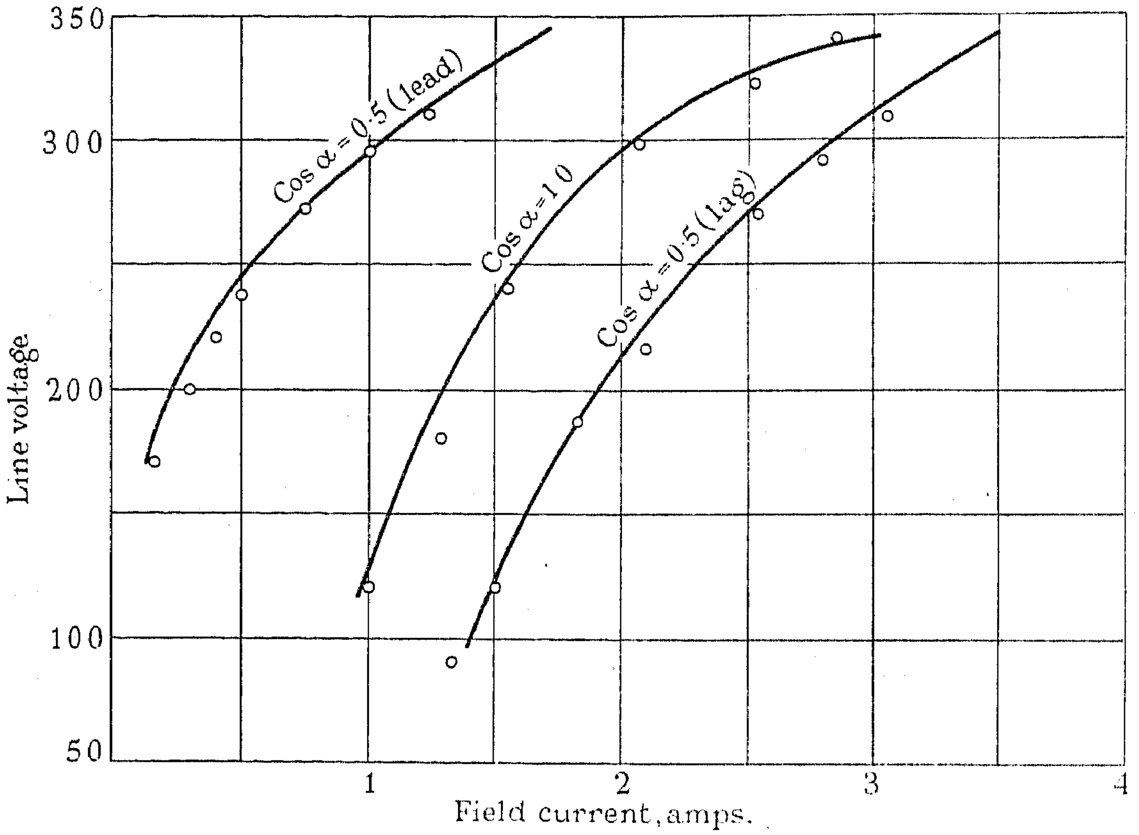

Fig. 18.—Machine 1, 20 amps.

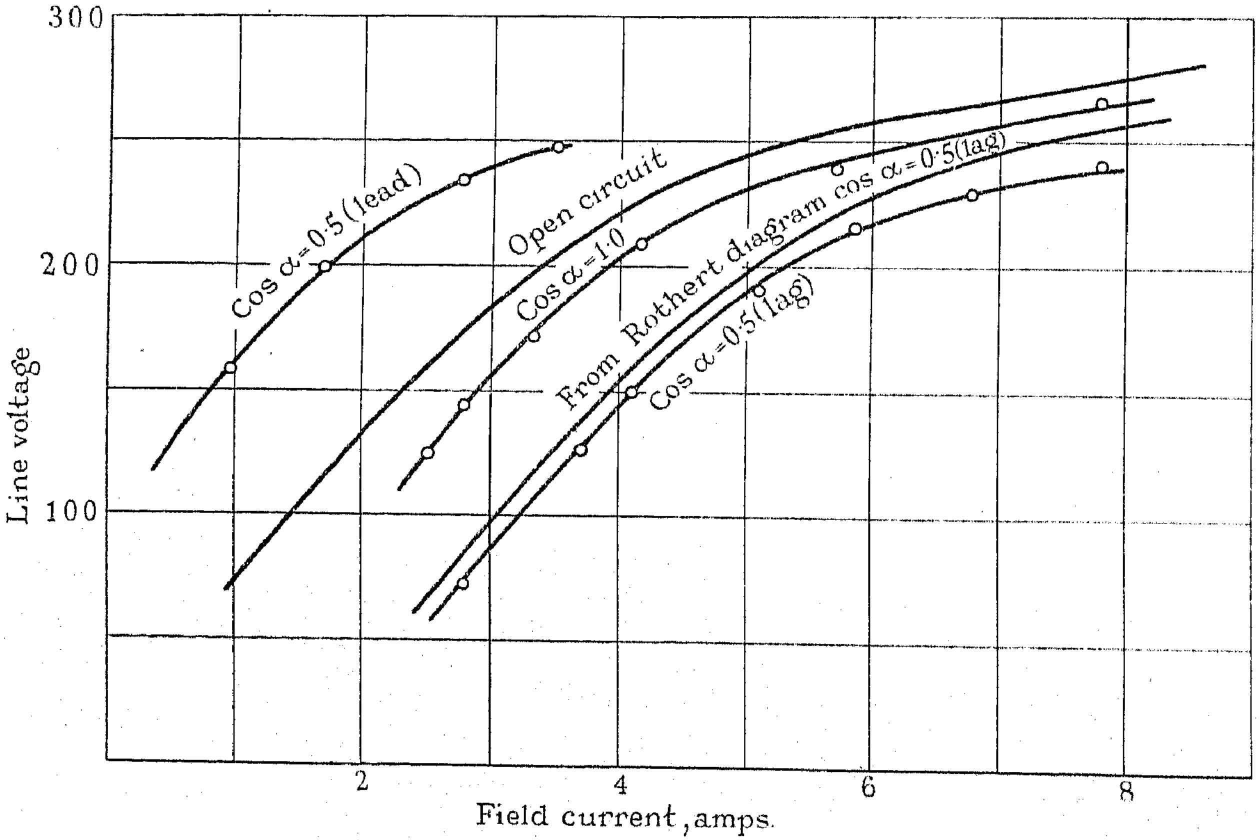

Fig. 19.—Machine 2, 41 amps.

lagging current as machine slips a pole without reversed field = 21 amps.

The value of ρ plotted against terminal voltage is given in Fig. 13.

At 200 volts $x_d = \dfrac{200}{5{\cdot}95 \times 3{\cdot}1\sqrt{3}} = 6{\cdot}36$ (demagnetizing) (I_L leading)

$x_d = \dfrac{200}{6{\cdot}2 \times 3{\cdot}1\sqrt{3}} = 6{\cdot}0$ (magnetizing) (I_L lagging)

From these particulars the relation between load angle and power for various excitations was plotted, using the formula

$$W = \sqrt{3}V$$

$$\left[\left(I_{fa}\sin\theta + I_0\left(\frac{\sigma - 1}{2}\right)\sin 2\theta \mp \frac{R}{x_i}(I_{fa}\cos\theta - I_0)\right)\right]$$

where the negative sign implies motor and the positive sign generator operation.

The curves for machines 1, 2, and 3, operating on infinite busbars of 200 volts are plotted in Figs. 15, 16A, 16B, and 17, respectively. On the same axes are also plotted observed points obtained from a stroboscopic measurement of load and load angle. The method of obtaining these points has already been described.* Each calculated curve has been corrected to the same zero as the corresponding curve defined by the experimental points.

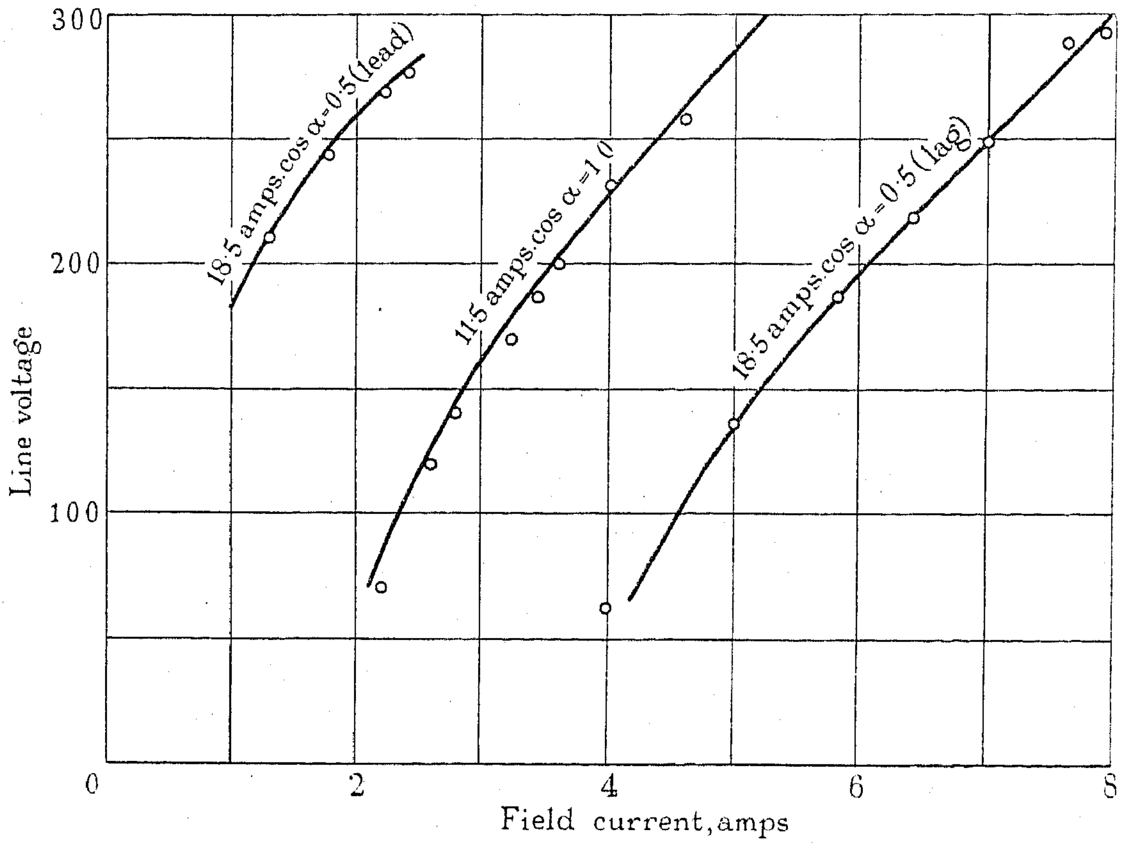

Fig. 20.—Machine 3.

The calculated and observed performance of the machines on busbars of variable voltage were compared by running each at constant armature current and power factor and varying the field excitation; after which terminal voltage was plotted against field current. The diagram of Fig. 8 was then used, with the correction for resistance, to calculate the field current for various assumed terminal voltages. The observed and calculated results are plotted for machines 1, 2, and 3, respectively, in Figs. 18, 19, and 20, the value of σ being assumed constant in each case.

In Fig. 19 the voltage curve obtained from the Rothert diagram for 41 amperes lagging current is also plotted for comparison.

APPENDIX

A somewhat fuller account is given here of the mathematical implications of the diagram for constant voltage.

For an asymmetrical machine, having resistance, equations (3) and (4) must be satisfied:—

$$I_1 = \sigma I_0 \sin\theta - \frac{R}{x_t}(I_{fa} - I_0\cos\theta) \quad . \quad . \quad (3)$$

$$I_2 = (I_{fa} - I_0\cos\theta) + \frac{R}{x_t}(I_0\sin\theta) \quad . \quad . \quad (4)$$

The co-ordinates of the extremity of the current vector are $x = I_2\cos\theta - I_1\sin\theta$; $y = I_1\cos\theta + I_2\sin\theta$. Moving the origin to the point $\left(I_0\frac{Z^2}{Z_t^2};\ I_0\frac{Z^2}{Z_t^2}\frac{R}{x_t}\right)$ we obtain the limaçon

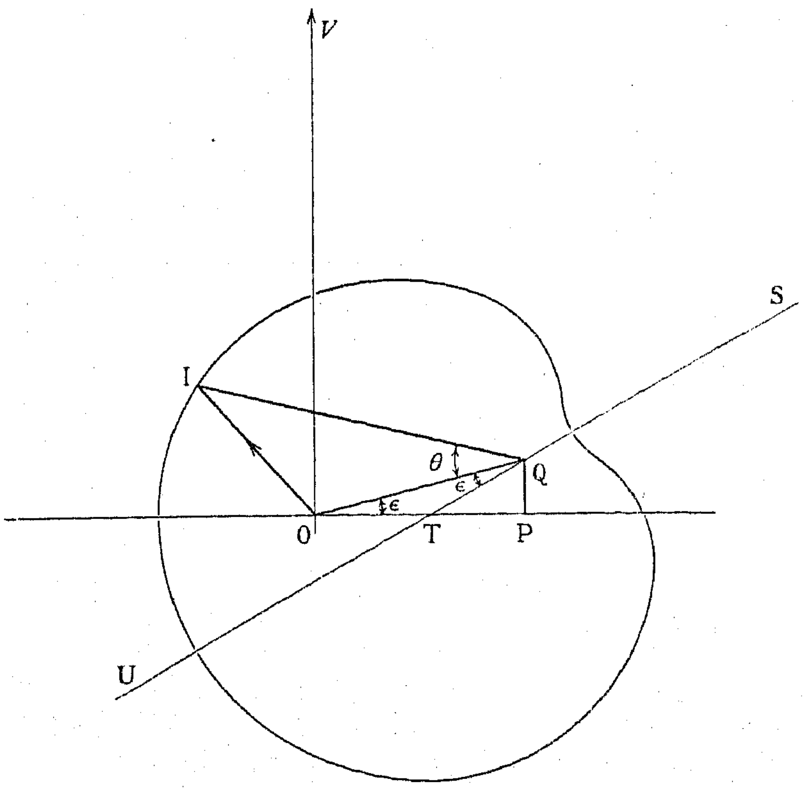

Fig. 21

$$r = I_{fa}\frac{Z_t}{x_t} + I_0(\sigma - 1)\cos(\theta + \epsilon)$$

where $Z_t = (R^2 + x_t^2)$; $I_{fa} = \rho I_f$; $I_0 = \rho I_{f_0}$; $\tan\epsilon = \frac{R}{x_t}$.

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* J. C. Prescott and E. W. Connon: "Measurement of the Load Angle of Synchronous Machinery," *Journal I.E.E.*, 1931, vol. 69, p. 281.

Taking the singular point as pole with co-ordinates $\left(I_0\frac{Z^2}{Z_t^2};\ I_0\frac{Z^2}{Z_t^2}\cdot\frac{R}{x_t}\right)$, the axis of symmetry of the figure makes an angle $\left[2 \text{ arc tan } (R/x_t)\right]$ with the x axis. Thus in Fig. 21 OP $= I_0(Z^2/Z_t^2)$, PQ $= I(Z^2/Z_t^2).(R/x_t)$, UQS is the axis of symmetry, and STP is 2 arc tan (R/x_t). Since QOP is arc tan $R/(xt)$, it follows that OQT $=$ QOP $= \epsilon$. The load angle θ may therefore be reckoned from the line OQ in a clockwise direction. OI is the load current and OV the applied voltage. If $R = 0$ the co-ordinates of the singular point are $(I_0\sigma;\ 0)$: $\epsilon = 0$ and the limaçon is given by

$$r = I_{fa} + I_0(\sigma - 1)\cos\theta$$

as can be seen at once from Fig. 5, since the excess of II_0' over I_2I_0 is always $I_0(\sigma - 1)\cos\theta$.

If $x_t = x_d = x$ the limaçon becomes a circle the co-ordinates of whose centre are $(I_0 : I_0R/x)$ and whose radius is $(Z/x)I_{fa}$. Expressing I_0 and I_{fa} in terms of terminal voltage and e.m.f. respectively, by introducing ρ, the quantities take the form in which they are quoted by Wall.

ACKNOWLEDGMENTS

The authors wish to express their thanks to Prof. E. W. Marchant, D.Sc., Past-President, for placing at their disposal machinery and apparatus in the Laboratories of Applied Electricity in the University of Liverpool, and for his encouragement and criticism; to Mr. U. S. Haslam-Jones, M.A., and Mr. S. D. Daymond, B.A., B.Sc., for their valuable help in the mathematical analysis; and to those Honours students in the University of Liverpool who have patiently assisted in obtaining the experimental results upon which the paper is founded.

DISCUSSION ON
"APPLICATIONS OF THE HOT-CATHODE GRID-CONTROLLED RECTIFIER, OR THYRATRON"*

SOUTH MIDLAND CENTRE, AT BIRMINGHAM, 30TH NOVEMBER, 1936

Mr. E. A. Reynolds: I should like to know how the word "thyratron" originated, who invented it, and what is it meaning.

The author mentions the use of this device in aluminium and aluminium-alloy welding, but from the way in which he speaks of the high current that is taken I gather that there is some difficulty in using the thyratron for the current required. If, however, it can be used for this purpose, it is obviously the most useful form of welding control that there can be for these alloys, because the period of weld is so extraordinarily short and many of the present forms of welding control have failed on aluminium alloys for that reason. It therefore looks as though the thyratron provides a possible solution of the difficulty of welding aluminium alloys.

Another point in the paper which is of particular interest is the control of theatre lighting. I understand that the apparatus which the author puts forward is very expensive in the first place as compared with the resistance control ordinarily used, but it is quite possible that the saving through not dissipating energy in the resistances might compensate for this. Against this, however, there is the life of the thyratron to consider, because when dimming of the lights is required it is impossible to wait for the thyratron to heat up, and therefore it must be in circuit the whole time the lights may be required. The introduction of the thyratron seems to open up a very useful method of lighting control in theatres because it is so easily controlled from any position without the use of bulky apparatus and of cables carrying heavy current.

Mr. J. H. Fearon: Has the possibility been considered of encasing thyratrons in metal, in the same way as radio valves have been encased? This might help to overcome some of the prejudice which is said to exist regarding the fragile nature of the apparatus.

Mr. H. H. Taylour: With regard to the name "thyratron," it is rather unfortunate that a term which is coming into such general use should be a trade name, inasmuch as this circumstance has brought into being a somewhat confusing alternative, namely "gasfilled relay." This latter term is unsatisfactory, as, apart from the undesirability of having widely dissimilar names for the same piece of apparatus, it is definitely inaccurate since the device is neither gasfilled (in its usual form) nor is it a relay. It is true that grid-controlled electronic devices containing gas at low pressure are known, but surely the term "gasfilled" should apply only to them since, particularly with regard to temperature characteristics, there are wide differences between them and those with a vapour filling. The word "relay" in engineering matters commonly refers to a mechanical device, while, in many instances, thyratrons are not employed as relays.

The type of voltage regulator described by the author has apparently been developed only to give a level compounded characteristic. There are, however, other characteristics which are sometimes required. For instance, regulators are frequently specified which bestow a rising characteristic with load, and occasionally regulators are required to combine this with a drooping characteristic in the overload region. Will the author give details of any experience he may have had of producing combined characteristics of this nature with the non-ohmic bridge regulator?

Mr. W. R. Cox: When the possibility was being examined of applying a hot-cathode grid-controlled rectifier to a number of control and protective services, two characteristics were found to be troublesome. The first was the fact that a heating-up period for a cathode is necessary, and the second the fact that the rectifier valves have a comparatively short life. Information on any developments which have occurred since the above investigations were made and which have led to improvements in regard to either of these points will be very welcome.

The first of the above characteristics makes it necessary to keep the cathode continually warm if the valve must always be ready for immediate use, as for protective services. The difficulties here introduced are, in the first place, to provide a suitable supply of heating current for the cathode, and, secondly, the cost of the energy consumed in this way.

At their present price, the limited life of the valves makes the question of replacements a relatively expensive item. It is, of course, realized that as the demand for these valves increases, their price is likely to fall very considerably, and this will greatly ease the situation from the point of view of replacements.

From a general point of view, the fact that we have in these hot-cathode grid-controlled rectifiers a relatively large switch, which is at the same time a static piece of apparatus, very rightly attracts a good deal of attention. Other attractive features are that the switches are capable of extremely high-speed operation and possess the characteristic that the speed of operation can be varied over a comparatively wide range with great precision. At the present stage of development their application still seems to be rather limited, the capital outlay and maintenance charges only being the limiting factors.

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* Paper by Mr. A. L. Whiteley (see vol. 78, page 516).

Mr. A. L. Whiteley (*in reply*): In reply to Mr. Reynolds's question regarding the name "thyratron," it is derived from the Greek word meaning "a door," together with the not uncommon word-ending signifying that it is a member of the electron-tube family. The word was coined by the Research Laboratory of the General Electric Co. at Schenectady. In view of the grid-control characteristic of the thyratron, the analogy with a door will be seen to be quite appropriate. Mr. Taylour also refers to the name of the device, and I entirely agree with him that the alternative name sometimes used, "gasfilled relay," is very often inappropriate. It is clearly incorrect to describe the thyratron as a relay when, as is often the case, it carries the whole of the power being controlled.

Referring to Mr. Reynolds's observation regarding welding of aluminium and aluminium alloys, the thyratron combined with the series transformer system enables large-kVA welding machines to be controlled. For instance, two of the largest thyratrons at present manufactured in this country, when placed across the 6·6-kV secondary of a series transformer can handle 300–350 kVA, i.e. about 30 000 amperes welding current. Developments now reaching their conclusion will enable this rating to be quadrupled, particularly in the case of spot welding.

Mr. Reynolds is correct in stating that the thyratron reactor dimmer is expensive compared with the usual resistance type. While the thyratron dimmer is more efficient than the resistance dimmer, it cannot be claimed that the saving in power will compensate for the increased capital cost. As Mr. Reynolds remarks, the thyratrons must be in circuit (i.e. cathodes hot) for the whole duration of the stage show. The power absorbed by the cathodes is, however, small (29 watts for circuits up to 6-kW capacity, and 49 watts for circuits of 6- to 20-kW capacity).

The main features of the thyratron reactor dimmer are (*a*) the ease with which a large number of stage circuits can be controlled by a single operator; and (*b*) the ability to achieve true scene presetting, i.e. the lighting intensity of any or all the stage circuits can be "set up" in advance to any degree of "dim." This is done without interfering with existing intensities of these stage circuits. When a change of scene is called for, the lighting intensities are "faded through" to the new values, by a single control knob. The transition period can be of any length from 1 sec. upwards; alternatively, the outgoing scene can be faded to black-out, and the new scene brought up from black-out, using two controls only. Those acquainted with the difficulties attendant upon the handling of a 60- or 100-circuit stage board will appreciate the convenience of performing all scene-changing on one or two easily-operated control knobs. The foregoing control features cannot be obtained with any known form of resistance dimmer. Thus I would suggest that, as lighting control is such an important feature of a modern stage production, a better perspective is gained if the increased cost of a thyratron dimmer is not reckoned as a percentage of the cost of a resistance dimmer but rather as a percentage of the cost of the stage equipment as a whole.

Mr. Fearon asks whether the possibility has been considered of enclosing thyratrons in metal. Very little has been done in this country with regard to metal thyratrons, although developments in this direction are now proceeding. In America, where thyratrons are in more common use, certain sizes have been available in welded steel envelopes for the last 2 or 3 years.

Regarding prejudices against thyratron control, I have no hesitation in stating that there is a more ready acceptance of this class of control than when the paper was first presented, about 18 months ago. There is no doubt that "all steel" thyratrons will do much to overcome the psychological difficulties which are present in introducing this new development; however, glass-enclosed thyratrons, particularly in the small sizes, are likely to be used for some time to come.

In reply to Mr. Taylour, thyratron regulators have been produced commercially only to maintain constant voltage. In the main, the effort has been to achieve very close limits of regulation, say 0·1 % from no load to full load on the generator. It is in this field that the electron-tube regulator shows up to great advantage. I would remind Mr. Taylour that the characteristic he describes is somewhat rare, and that a good enough approximation can often be achieved from the inherent characteristics of the machine itself.

I gather from Mr. Cox's remarks that he is interested in the use of thyratrons as protective devices for transmission systems. In this case I agree that the finding of a low-voltage a.c. supply for the hot cathodes may be troublesome. I cannot, however, agree with him with regard to the remaining factors, namely tube life, cost of cathode energy, and cost of replacements. In a protective device the thyratron cathode must always remain hot, but the arc will not be called upon to strike except under fault conditions. The life expectancy in such a service would be in the region of 10 000 hours. If the duty to be performed is a very important one, I would suggest the use of two thyratrons in parallel. The cathode consumption of a thyratron which is likely to be suitable for protective-relay work is only 13 watts. Maintenance costs are difficult to discuss in general terms, but if thyratrons can be shown to have improved characteristics for high-speed protective working the cost of replacing a few small thyratrons each year will be negligible compared with the maintenance cost of the moving electromechanical parts of the protective apparatus.

DISCUSSION ON
"CONTINUOUS EXTRUSION OF LEAD CABLE-SHEATHS"*

SCOTTISH CENTRE, AT GLASGOW, 9TH MARCH, 1937

Mr. A. F. Stevenson: I have met with no trouble due to split lead outside the factory, but have had some experience of it in the works with a design of die-box now quite out of date. My cable-using friends have so little trouble due to this cause that I feel that the author's machine meets no want on the technical side, though it may on the commercial side. I regret that while the paper is very thorough in proving that the old-type press cannot produce good sheathing, it gives no figures as to the relative cost by the two processes, which is the important point.

Mr. A. P. Robertson: I should be glad if the author would confirm that at the die the lead is in a plastic condition.

The cable before it is covered is soft, and I should like to know whether the pressure of 15 tons per sq. in. has any effect in reducing the diameter or otherwise distorting it. The temperature of 300° F. seems high, and might damage the insulation, although I must admit that I have seen no signs of this. Why is this so? Is it because of the speed of travel?

I am not clear as to the method adopted for keeping the cable central. Is the point the part of the machine which keeps the cable central, and the die for forming the lead outside? As to the screw in the lead, I take it that the lead must be fairly solid to give the screw a catch, and that the molten lead at the far end is only provided to fill up the screw so that it does get a proper catch.

Mr. W. Ross (Glasgow): I should like to ask how the cable is kept central when it is passing through the machine.

The author said that at the mouth of the die the temperature is about 400° F. Has this temperature any effect on the insulation?

What is the purpose of the splines on the die shank—is it to keep the lead from revolving? When that lead comes out, just before it enters the die is it in the form of a tube with internal splines and screwed outside? If it is, does not the irregular cross-section affect the lead sheath?

Mr. E. Seddon: I have had some bitter experiences with faulty lead-covering of cables, where the lead sheathing has fallen away from the cable in halves; and it is easy to see from a perusal of the paper how such faults can originate from the old type of lead press.

With regard to the regulation of speed on the receiving drum, I should like the author to say whether the amount of droop on the cable between the press and the drum can be used to control the speed, by allowing the cable to pass through a guide in the path of a beam of light operating a photocell.

Mr. C. R. Kemp: I should like to ask whether the author's machine can make ordinary lead pipes.

Mr. J. Eccles: I fear that the high temperature of the lead being deposited on the cable in the author's machine might have a deleterious effect on the outside of the insulation. The continuous-extrusion machine is rather worse in this respect than the old-type of press, as a uniform temperature of die is maintained approaching the peak value for the earlier type.

Has the Michell type of thrust bearing ever been tried in the author's machine? Perhaps the sliding speed is not high enough for this type, but it is an excellent bearing for the very high pressure mentioned by the author.

His system of speed stabilization seems to be rather complicated, and one would like to know why such refinements are necessary. The initial expense and running and maintenance costs of a Ward-Leonard set must offset a good deal of the commercial advantage claimed for the continuous-extrusion machine.

Mr. G. G. L. Preece: Some 25 years ago, when I was connected with the manufacture of cables, we had quite a lot of trouble inside the factory with split lead sheaths owing to the non-union of the two charges. We took precautions against this type of fault, but we could find no certain cure for it. The fact that such precautions *were* taken is proved by the small percentage of trouble due to this cause which has been experienced by users. One particular feature in those early days was the introduction of a hydraulic test for lead-sheathed paper cables, which enabled us to track faulty sheaths more effectively than by simple immersion; and, although not perhaps altogether successful in disclosing trouble, this drastic test was a measure of the amount of care that was taken then.

The success of the author's machine must be measured by the attitude of the cable makers towards it, which will be indicated by the rate of replacement of existing hydraulic lead-covering presses by the new type. Perhaps the author could give some information regarding this.

Mr. F. L. Smith: A serious defect of the older cable-sheaths was the patches of cracking which appeared after the material had been in use, and which—on rather slender evidence—were put down to fatigue. This evidence was that the defect was particularly noticed in positions where vibration might have had some effect on the cable, and it was thought that the trouble would be overcome by adding a small quantity of tin, which increased the fatigue limit. I should like to know whether this process has tended to eliminate that fault, and whether the alloy element is still employed.

Mr. H. C. Babb: Is the author's process of extrusion of the lead any better than the older process from a chemical-action point of view? In other words, does the placing of the lead over the core of the cable in this way tend to stop the chemical action which generally occurs, especially on unarmoured cables, where the atmosphere is impregnated with salt?

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* Paper by Dr. P. DUNSHEATH (see vol. 80, p. 353).

I was rather interested to note previous speakers' experience that few faults occur on cables. Of course, it takes quite a long time for faults to develop on lead-covered and armoured cables; nevertheless, I have come across several faults in cable sheaths caused by blowholes and splits. After hearing the author's explanation of how the cables are sheathed with ordinary machines, one can understand now how these faults occur.

Dr. P. Dunsheath (*in reply*): As in discussions on this paper elsewhere, speakers have in turn attacked and supported my views as to the existence of a split-lead problem. Mr. Stevenson's opinions, for example, are opposed to those advanced by Mr. Seddon, Mr. Babb, and others. Fortunately, it is not necessary to agree either that all existing cables are perfect or that, on the other hand, lead problems are serious. If the continuous lead-extrusion machine offers certain advantages explained in the paper, and, at the same time, definitely excludes oxide from the cable sheath, the point is not really important in the present discussion.

The question of relative cost of continuous and intermittent extrusion, raised by Mr. Stevenson, is one which depends to a large extent on local conditions. In at least one factory where this new method is in operation the labour has been reduced by one-third, although owing to differences occasioned by the nature of the product —manufacturing lengths, etc.—it would not be safe to generalize on this fact without longer experience. It can be stated, however, that the adoption of continuous lead extrusion may reasonably be expected to reduce costs.

In reply to Mr. Robertson, the lead at the die is in a plastic condition, but as the pressure is exerted on the lead in a longitudinal as distinct from a radial direction there is no resulting distortion of the cable core. Further, the die temperature does not result in damage to the dielectric, because the sheath cools off very rapidly on application to the cable. Mr. Eccles is not justified in claiming, however, that the continuous machine is worse than the press in this respect. Comparison of the curves of Figs. 10 and 11 is no criterion of the comparative temperatures of operation for the two processes. The curves are independently included to show the characteristic explained in the paper, and in practice the extrusion machine is usually run at a temperature approximating to the average of that obtaining in the non-continuous press. The assumptions by these and other speakers on the centralizing of the cable and the solidity of the lead, are both correct.

Mr. Ross is correct in assuming that the grooves at the front end are to prevent the lead revolving, but by special attention to the design of the die chamber the irregularities are prevented from interfering with the shape of the finished pipe.

Mr. Seddon's proposal for regulating the speed of take-up by means of a photo-electric cell has already been considered and provides a satisfactory system.

In reply to Mr. Kemp, the continuous lead-extrusion machine produces entirely satisfactory pipe, and at the present time an installation is being carried out in the works of a well-known pipe manufacturer.

On the mechanical features mentioned by Mr. Eccles, the Michell thrust bearing has not been tried owing to the need for starting up the machine under load, and the Ward–Leonard drive does seem to give a very desirable flexibility of operation without unduly excessive installation or running costs.

Mr. Preece's query regarding the rate of replacement of hydraulic presses in cable factories will naturally take a few years to answer. For the present the machines are only being installed for extensions, but if they justify their adoption, then replacement can only be a matter of time.

In replying to Mr. Smith and Mr. Babb, the adoption of continuous lead extrusion does not reduce the risk of damage due to corrosion and fatigue, although it is now definitely established that the latter can be met by the adoption of a suitable alloy.

INSTITUTION NOTES

NOMINATIONS FOR ELECTION TO THE COUNCIL

In addition to those Members nominated by the Council (see vol. 80, page 678) the following have been nominated for ballot as Ordinary Members of Council:—

C. RODGERS, O.B.E., B.Sc., B.Eng. [Nominated by P. R. Coursey, B.Sc.(Eng.), A. G. Ellis, A. B. Field, M.A., B.Sc., G. H. Fletcher, C. C. Garrard, Ph.D., R. E. Grime, M.Sc., R. G. Jakeman, D.Sc., H. D. Symons, F. Wallis, and W. Wilson, D.Sc., B.E.]

O. C. WAYGOOD [Nominated by L. Breach, J. Cormack, B.Sc., R. G. Devey, A. C. Livesey, G. K. Paton, P. J. Robinson, M.Eng., F. E. Spencer, E. G. Taylor, Prof. F. J. Teago, D.Sc., and B. Welbourn, M.Eng.]

OVERSEAS MEMBERS AND THE INSTITUTION

During the period 1st May to 30th June, 1937, the following members from overseas called at The Institution and signed the "Attendance Register of Overseas Members":—

Andrews, W. J. (*Buenos Aires*).
Briggs, W. T. (*Cairo*).
Cassé, C. W. (*Lucknow*).
Chowla, S. N. (*Bombay*).
Dunlop, R. P., B.Sc. (*Hong Kong*).
Finding, W. (*Abadan*).
Finnis, A. H. (*Tanganyika*).
Galea, R. F. (*Malta*).
Giles, S. R. (*Durban*).
Jenkins, J. S. (*Singapore*).
MacMaster, I. J., B.Sc. (*Cawnpore*).
Macnee, D. H., B.Sc. (*Sydney*).
Martin, A. J. H. (*Colombo*).
Patterson, H. J. N. (*Hamilton, N.Z.*).
Richardson, W. E. (*Durban*).
Scott, R. H. (*Buenos Aires*).
Shankariah, N. S., B.Sc. (*Bhadravarti*).
Sharpley, Prof. F. W., F.R.S.E. (*Dhanbaid*).
Simpson, G. R. (*Dunedin*).
Sutherland, J. G. A. (*Singapore*).
Taylor, W. T. (*Gold Coast Colony*).
Thomson, M. F. (*Jogindar Naggar, Punjab*).
Tozer, A. K. (*Sarafand, Palestine*).
Tucker, J. (*Queenstown, S. Africa*).
Verma, P. L., B.Sc. (*Bombay*).
Wall, Prof. W. G. P. (*Allahabad*).
Willoughby, G. P., M.Sc. Tech. (*Nairobi*).

JOINT COMMITTEE ON MATERIALS AND THEIR TESTING

The Institution has been asked to draw the attention of its members to the first technical discussion to be held under the auspices of the Joint Committee. The discussion will take place at Manchester on the afternoon and evening of Friday, the 29th October, 1937, when papers on "Notched Bar Impact Testing" will be presented by Mr. M. Moser (of Messrs. Krupp, Essen), Mr. L. W. Schuster (British Engine Boiler and Electrical Insurance Co)., and Prof. R. V. Southwell (University of Oxford). The meeting will be held in the Large Hall of the College of Technology, Manchester, commencing at 2.45 p.m.

Applications for admission to the meeting and for advance copies of the papers (for which a small charge will be made) should be forwarded to Mr. T. Makemson, Secretary, Manchester Association of Engineers, St. John Street Chambers, Deansgate, Manchester, 3.

TRANSFERS

The following transfers were effected by the Council at their meeting held on the 1st July, 1937:—

Student to Graduate

Brooke, Ralph Crichton R.
Chown, John Charles H.
Cooper, Denis Ralph, B.Sc.
Fenn, Robert William.
Goedhals, Gerard, B.Sc. (Eng.).
Hands, Thomas.
Harris, Evelyn Thomas C., B.Sc.(Eng.).
Hatton, Kenneth Norman.
Lamping, Charles Thomas.
Liddle, John.
Loader, Herbert Marston.
Miller, Frederick William.
Miller, Geoffrey Martin.
Panton, Joseph Dennis.
Piggott, John, B.Sc.(Eng.).
Rajappa, S. V.
Rimmer, George Mervyn.
Rumsey, Robert William.
Sahgal, Shiam Sunder L.
Schunker, Charles Victor.
Shohet, Joseph Isaac.
Slaffer, Marks, B.Sc.
Smith, Ivor Ferguson.
Sparke, Jack Welton, B.Sc.
Stannard, George, B.Sc.
Styles, Frederick Robert, B.Sc.
Theakstone, Thomas Ross.
Thomson, Harry.
Tremain, Alfred Charles, B.Sc.(Eng.).
Waldron, Stephen Bowen.

ACCESSIONS TO THE REFERENCE LIBRARY

[NOTE.—The books cannot be purchased at The Institution; the names of the publishers and the prices are given only for the convenience of members; (*) denotes that the book is also in the Lending Library.]

AGNEW, W. A. Electric trains: their equipment and operation. Including notes on electric locomotives, electro-pneumatic brakes, regenerative braking, air-operated doors, etc. 2 vol. x + 1–220, x + 221–506 pp. 4to. (London: Virtue and Co., 1937.) 40*s*. (*)

ANNETT, F. A., and ROE, A. C. Connecting and testing direct-current machines. 2nd ed. 8vo. xiii + 302 pp. (New York, London: McGraw-Hill Publishing Co., Ltd., 1937.) 18*s*. (*)

ARMSTRONG, H. C., and LEWIS, C. V. Boiler management, maintenance and inspection: a handbook for boiler owners and boiler attendants. sm. 8vo. xii + 160 pp. (London: Charles Griffin and Co., Ltd., 1937.). 4*s*. (*)

ARNOLD, E., and LA COUR, J. L. Die Wechselstromtechnik. 3e Aufl. Band 2, Die Transformatoren: ihre Theorie, Konstruktion, Berechnung und Arbeitsweise. Von J. L. La Cour und K. Faye-Hansen. 8vo. xv + 699 pp. (Berlin: Julius Springer, 1936.) *RM*. 48.

ASPINALL, H. T. Worked examples in electrotechnics. pt. 2. sm. 8vo. viii + 207 pp. (S. Africa: Central News Agency, Ltd., 1937.) 7*s*. (*)

ATKINS, E. A., *M.Sc.*, and WALKER, A. G. Electric arc and oxy-acetylene welding. A practical handbook for works managers, welding operators and students. 3rd ed. 8vo. x + 394 pp. (London: Sir Isaac Pitman and Sons, Ltd., 1936.) 8*s.* 6*d.* (*)

Bauer, F. Der Kondensator in der Starkstromtechnik. 8vo. vi + 214 pp. (Berlin: Julius Springer, 1934.) *RM.* 18.50.

Bell, L. M. T. The making and moulding of plastics. 8vo. xviii + 238 pp. (London: Hutchinson's Scientific and Technical Publications, 1936.) 12*s.* 6*d.* (*)

Blalock, G. C. Principles of electrical engineering: theory and practice. 8vo. xix + 584 pp. (New York, London: McGraw-Hill Publishing Co., Ltd., 1936.) 24*s.* (*)

Bragg, W. L., *M.A.*, *Sc.D.*, *F.R.S.* Electricity. 8vo. xi + 286 pp. (London: G. Bell and Sons, Ltd., 1936.) 8*s.* 6*d.* (*)

British Broadcasting Corporation. B.B.C. Annual, 1937. la. 8vo. 176 pp. (London: B.B.C., 1937.) 2*s.* 6*d.*

Brown, H. G. The lead storage battery. 3rd ed. 202 pp. (London: The Locomotive Publishing Co., Ltd., 1937.) 5*s.* (*)

Brüche, E., and Scherzer, O. Geometrische Elektronenoptik. Grundlagen und Anwendungen 8vo. xi + 332 pp. (Berlin: Julius Springer, 1934.) *RM.* 28.40.

Bureau de l'Union Internationale des Télécommunications. Liste alphabétique des indicatifs d'appel des stations terrestres, mobiles et fixes. 6e éd. Mai, 1936. [*with Supplements.*] la. 8vo. 210 pp. (Berne: B. de l'U. I. des T., 1936.) 10 *francs.*

Cable Research. Handbook no. 3. Telephone cable practice. obl. 8vo. *pagin. var.* (London: Commercial Secretaries Ltd., 1936.) 40*s.* (*)

Carr, T. H. Power station electrical equipment and layout. The Association of Engineering and Shipbuilding Draughtsmen, session 1936–37. 8vo. 102 pp. (London: The Draughtsmen Publishing Co., Ltd., 1936.) 4*s.* (*)

Copper Development Association. Publication no. 22. Copper for bus-bar purposes. [By G. W. Preston.] 8vo. 161 pp. (London: Copper Development Association, 1936.)

Cotton, H., *M.B.E.*, *D.Sc.* The transmission and distribution of electrical energy. 8vo. ix + 452 pp. (London: The English Universities Press Ltd., 1937.) 21*s.* (*)

Cross, H. H. V., *Ph.D.* Electricity in therapeutics. Including special sections on modern electrodiagnosis and short-wave currents. 8vo. xxv + 380 pp. (London: Crosby Lockwood and Son, Ltd., 1936.) 25*s.* (*)

Curtis, H. L., *Ph.D.* Electrical measurements. Precise comparisons of standards and absolute determinations of the units. 8vo. xiv + 302 pp. (New York, London: McGraw-Hill Publishing Co., Ltd., 1937.) 24*s.* (*)

Dahr, K. A course of integrational and operational calculus, with applications to problems of physics and electrotechnics. 8vo. 169 pp. (Stockholm: Henrik Lindståhl, 1935.) 10*s.* 6*d.*

Davidge, H. T., and Hutchinson, R. W., *M.Sc.* Technical electricity. 5th ed. 8vo. x + 520 pp. (London: University Tutorial Press, Ltd., 1936.) 12*s.* 6*d.* (*)

Dickinson, H. W. Matthew Boulton. 8vo. xiv + 218 pp. (Cambridge: University Press, 1935.) 10*s.* 6*d.*

Dover, A. T. Power wiring diagrams. A handbook of connection diagrams of control and protective systems for industrial plants. 3rd ed. sm. 8vo. xvi + 216 pp. (London: Sir Isaac Pitman and Sons, Ltd., 1936.) 6*s.* (*)

Dudley, A. M., *M.S.* Connecting induction motors. 3rd ed. xv + 464 pp. (New York, London: McGraw-Hill Publishing Co., Ltd., 1936.) 21*s.* (*)

Electricity Commissioners. Electricity Supply Regulations, 1937, for Securing the Safety of the Public and for Insuring a Proper and Sufficient Supply of Electrical Energy. 8vo. 30 pp. (London: H.M.S.O., 1936.) 6*d.*

—— Electricity Supply Regulations, 1937. Explanatory notes . . . . prepared by the Electricity Commissioners. 8vo. 16 pp. (London: H.M.S.O., 1936.) 3*d.*

Everitt, W. L., *Ph.D.* Communication engineering. 2nd ed. x + 727 pp. (New York, London: McGraw-Hill Publishing Co., Ltd., 1937.) 30*s.* (*)

Fröhlich, H., *Dr.-Ing.* Beitrag zur Berechnung von Mastfundamenten. 3e Aufl. 8vo. viii + 81 pp. (Berlin: Wilhelm Ernst und Sohn, 1936.) *RM.* 6.65.

Greenland, S. F. Hot water service: design and pipe sizing. sm. 8vo. 189 pp. (London: G. A. Philpot, Ltd., 1937.) 12*s.* 6*d.*

Griffiths, E., *D.Sc.* Pyrometers. Recent developments in pyrometric appliances and methods for calibrating temperature-measuring instruments, with notes on electric furnaces. sm. 8vo. xi + 126 pp. (London: Sir Isaac Pitman and Sons, Ltd., 1926.) 7*s.* 6*d.*

Güntherschulze, A., *Dr.-Ing.*, and Betz, H. Elektrolytkondensatoren: ihre Entwicklung, wissenschaftliche Grundlage, Herstellung, Messung und Verwendung. 8vo. 178 pp. (Berlin: Verlag von M. Krayn, 1937.) *RM.* 16.

Hague, B., *D.Sc.*, *Ph.D.* Instrument transformers, their theory, characteristics and testing. A theoretical and practical handbook for test-rooms and research laboratories. 8vo. xxiv + 656 pp. (London: Sir Isaac Pitman and Sons, Ltd., 1936.) 35*s.* (*)

Hall, G. H., *M.E.* Motor and control applications. 8vo. viii + 259 pp. (New York, London: McGraw-Hill Publishing Co., Ltd., 1937.) 18*s.* (*)

Henney, K. Electron tubes in industry. 2nd ed. 8vo. viii + 539 pp. (New York, London: McGraw-Hill Publishing Co., Ltd., 1937.) 30*s.* (*)

Herring, J. M., *Ph.D.*, and Gross, G. C. Telecommunications: economics and regulation. 8vo. x + 544 pp. (New York, London: McGraw-Hill Book Co., Inc., 1936.) 30*s.*

International Electrotechnical Commission. I.E.C. specification for tests on impregnated paper-insulated lead-covered cables for voltages of 10 kV to 66 kV. 4to. 13 pp. (London: I.E.C., 1936.) 2*s.*

Issarte, H. Les dangers des courants électriques. 8e éd. 8vo. 81 pp. (Paris: Léon Eyrolles, 1936.) 10 *francs.*

Jørgensen, M. O. Experimental investigations regarding the applicability of Lichtenberg figures to voltage measurement. (Ingeniørvidenskabelige Skrifter, A, nr. 37.) la. 8vo. 39 pp., 16 pl. (Copenhagen: Danmarks Naturvidenskabelige Samfund, 1934.) *Kr.* 2,00.

Karlson, P. You and the universe: modern physics for everybody. Translated by B. Miall. 8vo. 325 pp. (London: George Allen and Unwin, Ltd., 1936.) 12*s.* 6*d.* (*)

Kehse, W. Die Hochspannungstechnik der Transformatoren, Isolatoren und Durchführungen. x + 146 pp. (Stuttgart: Ferdinand Enke, 1937.) *RM.* 11.

—— Der praktische Transformatorenbau. Eine Anleitung für Entwurf und Werkausführung. 8vo. xii + 109 pp. (Stuttgart: Ferdinand Enke, 1934.) *RM.* 8.80.

Kemp, P., *M.Sc.* The alternating current. 2nd ed. sm. 8vo. viii + 104 pp. (London: Sir Isaac Pitman and Sons, Ltd., 1936.) 2*s.* 6*d.* (*)

Koller, L. R., *Ph.D.* The physics of electron tubes. 2nd ed. 8vo. xvii + 234 pp. (New York, London: McGraw-Hill Publishing Co., Ltd., 1937.) 18*s.* (*)

Le Corbeiller, P. Electro-acoustique. 8vo. 89 + iv pp. (Paris: Etienne Chiron, 1934.) 18 *francs.*

Lewitt, E. H. Thermodynamics applied to heat engines. A textbook covering the syllabuses of the B.Sc.(Eng.) and I.Mech.E. examinations in this subject. 2nd ed. 8vo. x + 376 pp. (London: Sir Isaac Pitman and Sons, Ltd., 1937.) 12*s.* 6*d.* (*)

Lincoln Electric Company. Procedure handbook of arc welding: design and practice. 4th ed. 8vo. xii + 819 pp. (Cleveland, Ohio, U.S.A., and Welwyn Garden City, Herts: The Lincoln Electric Co., 1936.) 10*s.* (*)

Maycock, W. P. Electric wiring diagrams. 2nd ed., revised by H. C. Fabian. sm. 8vo. xii + 155 pp. (London: Sir Isaac Pitman and Sons, Ltd., 1937.) 5*s.* (*)

Milne-Thomson, L. M., *M.A.*, and Comrie, L. J., *M.A.*, *Ph.D.* Standard four-figure mathematical tables. Edition A and B. 2 vol. la. 8vo. (London: Macmillan and Co., Ltd., 1931.)

Edition A with positive characteristics in the logarithms. xvi + 245 pp. 10*s.* 6*d.*
Edition B, with negative characteristics in the logarithms. xvi + 243 pp. 10*s.* 6*d.*

Moon, P. The scientific basis of illuminating engineering. 8vo. xii + 608 pp. (New York, London: McGraw-Hill Publishing Co., Inc., 1936.) 30*s.* (*)

Morse, P. M. Vibration and sound. 8vo. xv + 351 pp. (New York, London: McGraw-Hill Publishing Co., Ltd., 1936.) 24*s.* (*)

Moseley, S. A., and McKay, H. Television: a guide for the amateur. 8vo. 144 pp. (London: Oxford University Press, 1937.) 5*s.* (*)

Moyer, J. A., and Wostrel, J. F. Industrial electricity and wiring. 2nd ed. 8vo. ix + 502 pp. (New York, London: McGraw-Hill Publishing Co., Ltd., 1937.) 15*s.* (*)

Nedden, F. zur, *Dr.-Ing.* Der Wert der Wärmeersparnis erläutert an der elektrowirtschaftlichen Gesamtstatistik Deutschlands und der Vereinigten Staaten von Amerika, 1912–1934. Ein betriebswirtschaftlicher Beitrag zur Kostendynamik. 8vo. 163 pp. (München, Berlin: R. Oldenbourg, 1936.) *RM.* 8.

Oldenbourg, R., *publisher.* Oldenbourg's practical charts. Tables for heat engineers: steam boiler operation. With explanation in . . . English-German-French. (London: The Technical Press, Ltd., 1936.) 7*s.* 6*d.*

Palm, A. Elektrische Messgeräte und Messeinrichtungen. 8vo. ix + 231 pp. (Berlin: Julius Springer, 1937.) *RM.* 16.

Parr, G. The low voltage cathode ray tube and its applications. 8vo. x + 177 pp. (London: Chapman and Hall, Ltd., 1937.) 10*s.* 6*d.*

Pender, H., *Ph.D.*, *Sc.D.* Electrical engineers' handbook. Wiley engineering handbook series, vol. 4, 5. Prepared by a staff of specialists under the editorship of H. Pender, W. A. Del Mar, K. McIlwain. 3rd ed. 8vo. (New York: John Wiley & Sons, Inc.; London: Chapman and Hall, Ltd., 1936.) (*)

vol. 4, Electric power. *pagin. var.* 30*s.*
vol. 5, Electric communication and electronics. *pagin. var.* 25*s.*

Political and Economic Planning. Report on the supply of electricity in Great Britain. A survey of present-day problems of the industry with proposals for reorganisation of electricity distribution. December, 1936. 4to. 171 pp. (London: P.E.P., 1936.) 8*s.* 6*d.*

Purday, H. F. P. Diesel engine design. 4th ed. 8vo. xix + 520 pp. (London: Constable and Co., Ltd., 1937.) 24*s.* (*)

Ramsey, A. S., *M.A.* Electricity and magnetism. An introduction to the mathematical theory. 8vo. x + 267 pp. (Cambridge: University Press, 1937.) 10*s.* 6*d.* (*)

Ranson, R. R. Electric meters. A practical treatise on D.C. and A.C. meters, measurements of power and resistance, reading and testing of watt-hour and graphic meters. 8vo. 232 pp. (Chicago: American Technical Society, 1934.) 10*s.* (*)

Retzow, U., *Dr.* Elektrotechnik und Witterung. 8vo. v + 121 pp. (Berlin: Julius Springer, 1936.) *RM.* 6.60.

Richter, R., *Dr.-Ing.* Elektrische Maschinen. Bd. 4, Die Induktionsmaschinen. 8vo. x + 440 pp. (Berlin: Julius Springer, 1936.) *RM.* 30.

Rider, J. F. The cathode-ray tube at work. 8vo. ix + 333 pp. (New York: John F. Rider, 1936.) 10*s.* 6*d.* (*)

Schwerdtfeger, W., *Dr.* Elektrische Messtechnik: 1, Gleichstrommesstechnik. (Lehrbücher der Feinwerktechnik; herausgegeben von K. Gehlhoff, Bd. 2). 8vo. viii + 199 pp. (Leipzig: C. F. Winter'sche Verlagshandlung, 1937.) *RM.* 5.20.

Smith, S. P., *D.Sc.* The electrical equipment of automobiles. 3rd ed. sm. 8vo. xii + 250 pp. (London: Chapman and Hall, Ltd., 1937.) 6*s*. (*)

Smithells, C. J., *D.Sc.* Tungsten: a treatise on its metallurgy, properties and applications. 2nd ed. 8vo. viii + 272 pp. (London: Chapman and Hall, Ltd., 1936.) 25*s*. (*)

Spon, E. and F. N., Ltd. Spons' electrical pocket-book. A reference book of general electrical information, formulae and tables for practical engineers. 6th ed., by W. H. Molesworth and G. W. Stubbings. sm. 8vo. viii + 400 pp. (London: E. and F. N. Spon, Ltd., 1937.) 6*s*.

Stafford, H. E. Troubles of electrical equipment: their symptoms, causes, and remedy. 8vo. ix + 330 pp. (New York, London: McGraw-Hill Publishing Co., Ltd., 1937.) 18*s*. (*)

Stubbings, G. W. Commercial a.c. measurements. 2nd ed. 8vo. xvi + 348 pp. (London: Chapman and Hall, Ltd., 1937.) 15*s*. (*)

Taylor, E. O. Utilisation of electric energy. 8vo. xi + 342 pp. (London: The English Universities Press, Ltd., 1937.) 21*s*. (*)

Taylor, F. S., *Ph.D.*, *M.A.* The world of science. 8vo. xvi + 1064 pp. (London: William Heinemann, Ltd., 1936.) 8*s*. 6*d*. (*)

Timbie, W. H. Elements of electricity. 3rd ed. 8vo. x + 569 pp. (New York: John Wiley & Sons, Inc.; London: Chapman and Hall, Ltd., 1937.) 15*s*. (*)

Todd, R. W., and Thompson, W. H. Outdoor high voltage switchgear. With a foreword by A. P. M. Fleming. 8vo. xii + 436 pp. (London: Sir Isaac Pitman and Sons, Ltd., 1937.) 42*s*. (*)

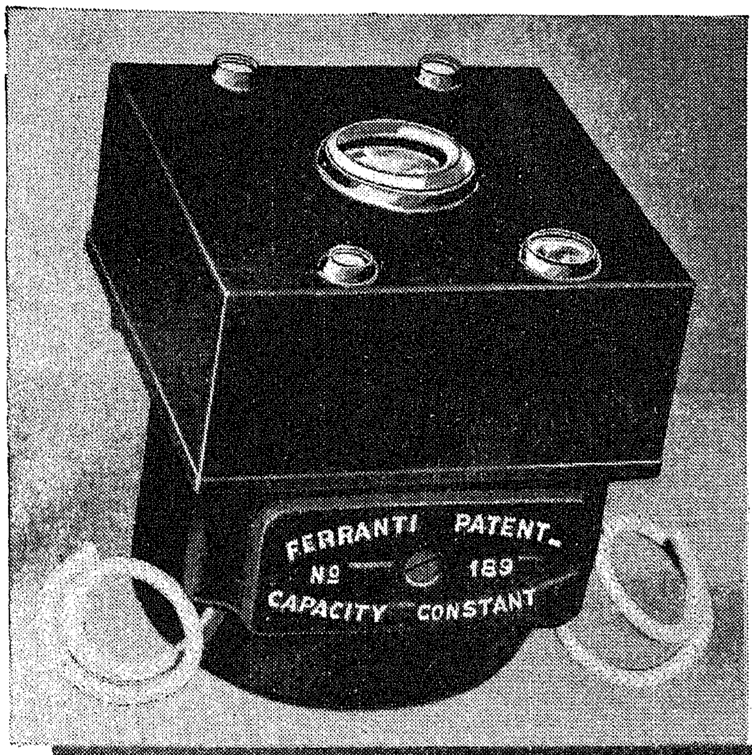
Varley, R., and Varley, T. W. The electro magnet. With contributions by A. E. Kennelly and E. Heitman. 3rd ed. sm. 8vo. A–P + viii + 175 pp. (Englewood, N.J.: Richard Varley, 1936.) $5.00.

Vilbig, F., *Dr.-Ing.* Lehrbuch der Hochfrequenztechnik. 8vo. xix + 775 pp. (Leipzig: Akademische Verlagsgesellschaft m.b.H., 1937.) *RM.* 32.80.

Widlake, H. C. Electricity on gas works. 8vo. xi + 186 pp. (London: Walter King, Ltd., 1930.) 21*s*. (*)

Wilson, A. H., *M.A.* The theory of metals. Based on an essay awarded the Adams Prize in the University of Cambridge 1931–1932. 8vo. viii + 272 pp. (Cambridge: University Press, 1936.) 18*s*.

Witts, A. T. Thermionic valves in modern radio receivers. A handbook of theory and practice. 8vo. x + 192 pp. (London: Sir Isaac Pitman and Sons, Ltd., 1937.) 8*s*. 6*d*. (*)



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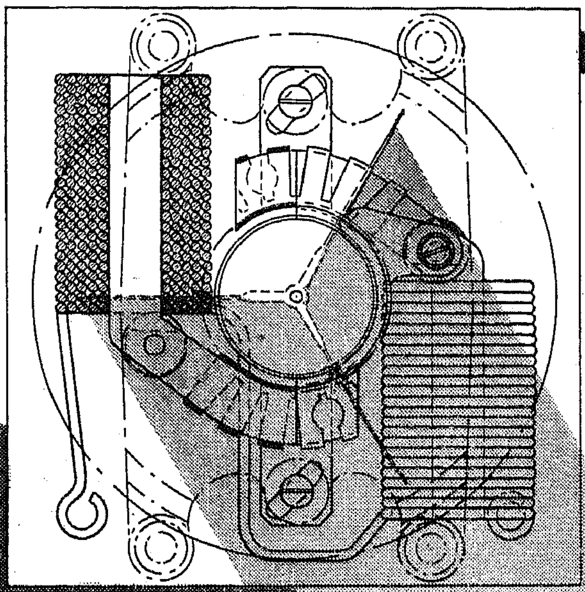
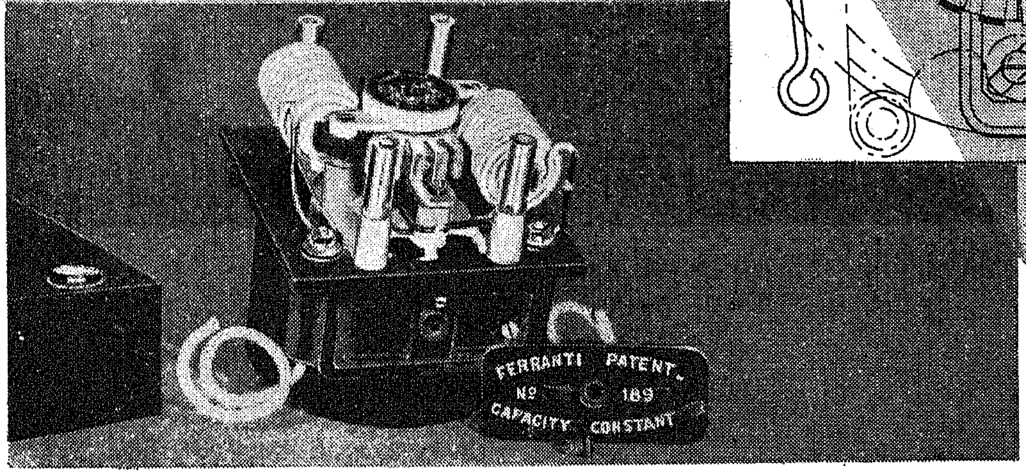
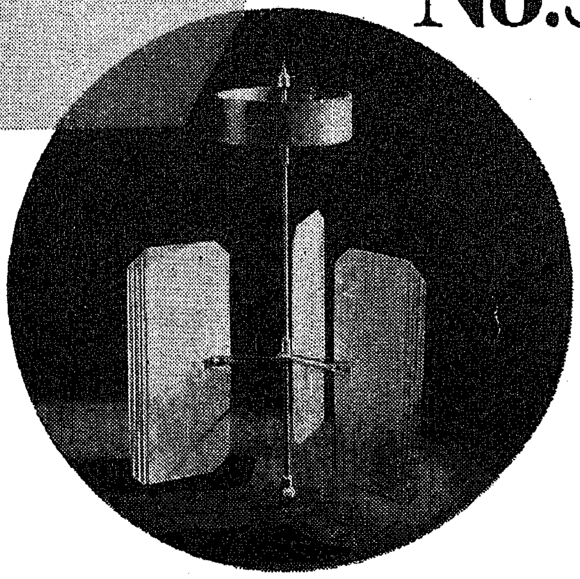


...Pioneer work by Ferranti in Electric Metering

No.3





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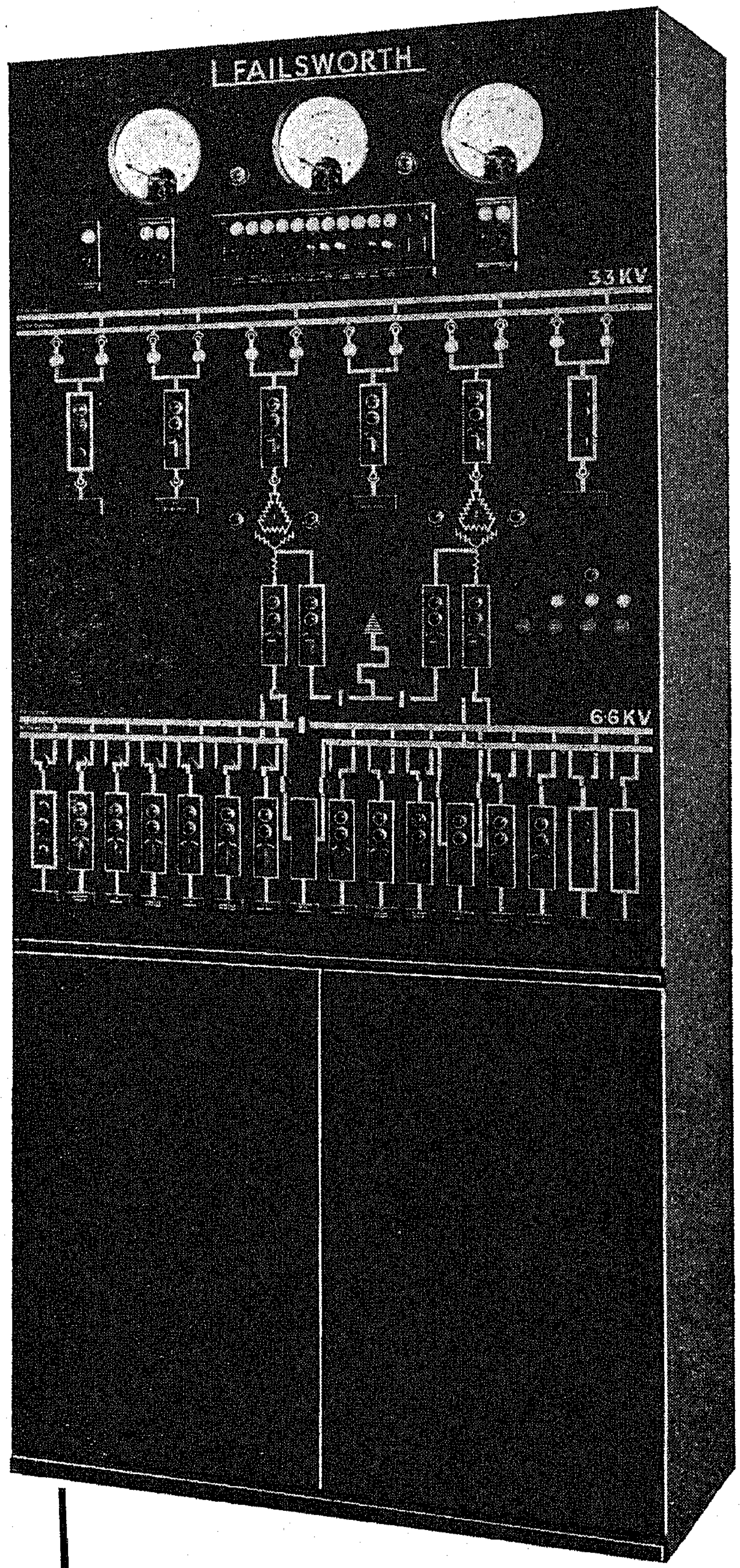

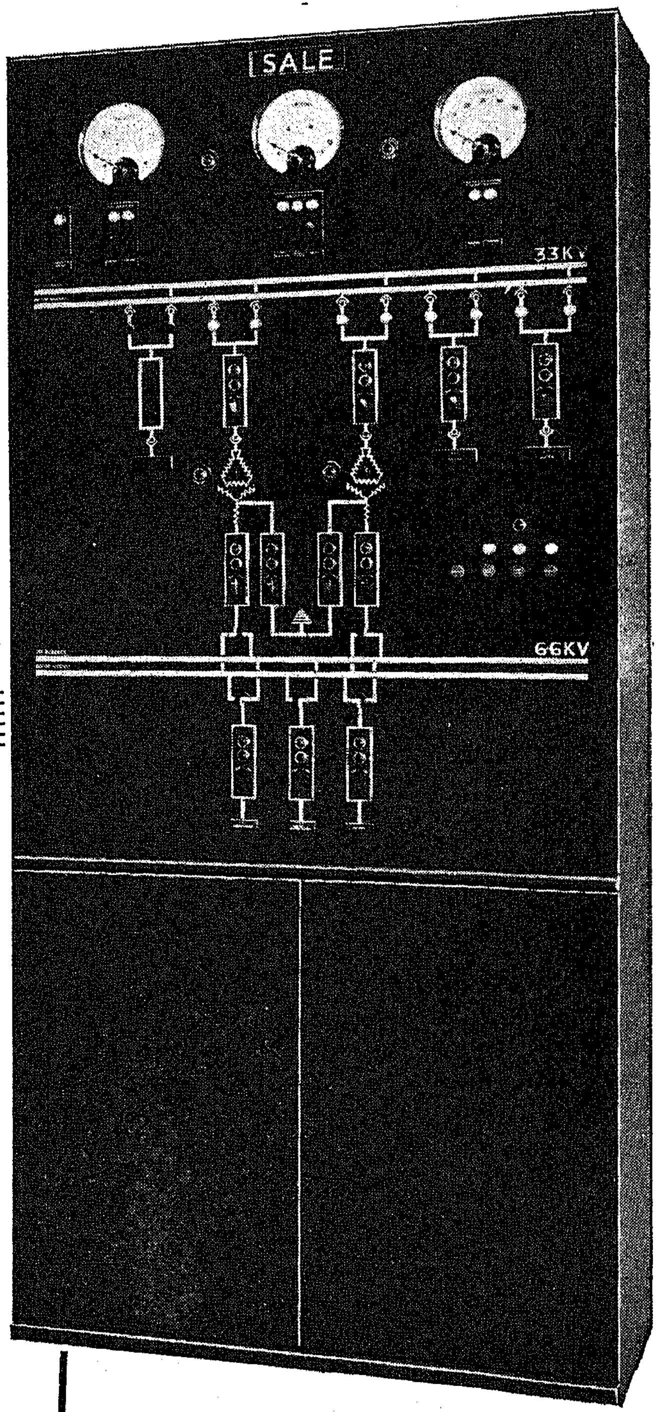

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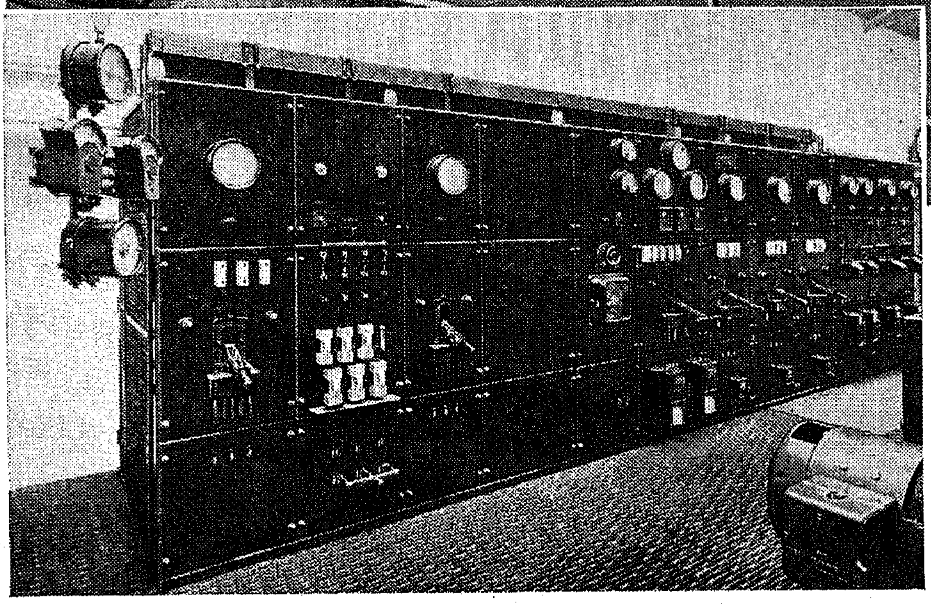

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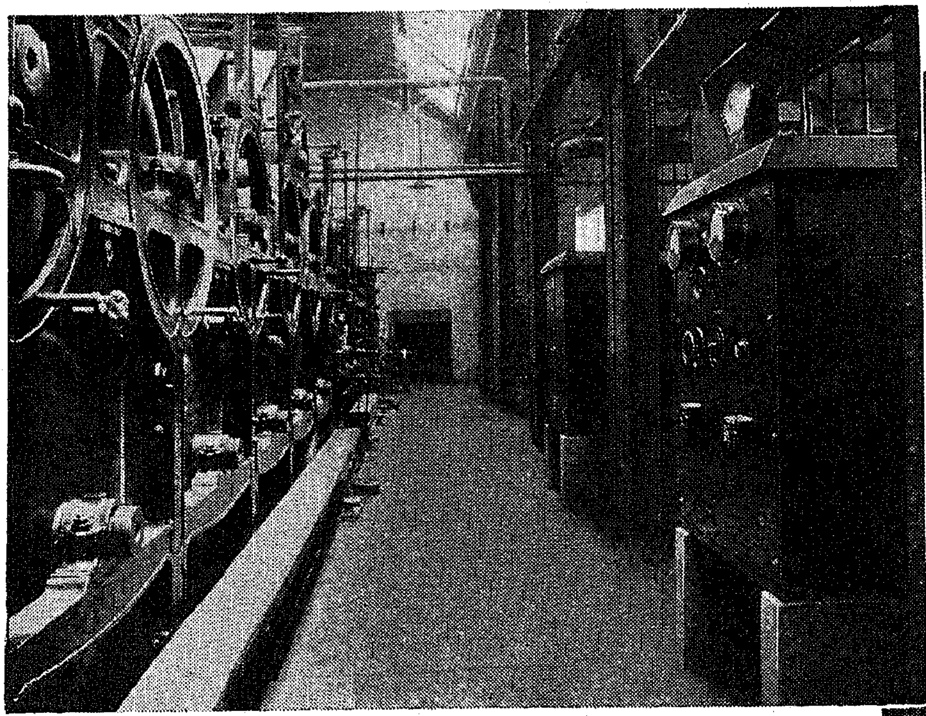
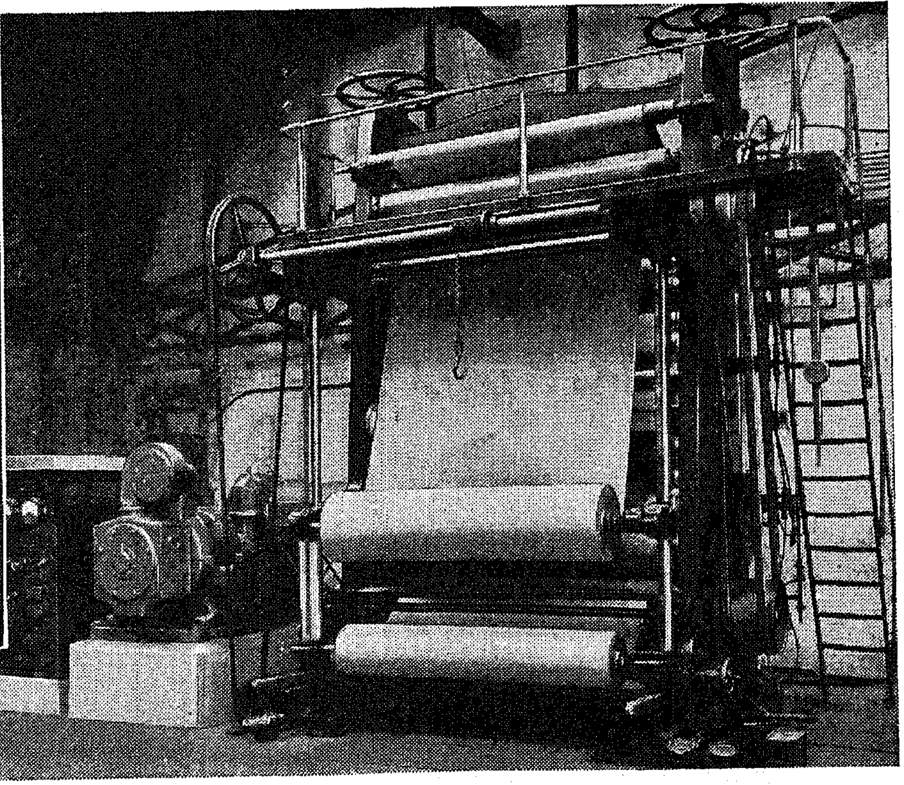


View showing one of the paper-making machines (on the left) and main control pillars for two paper-making machines (right)



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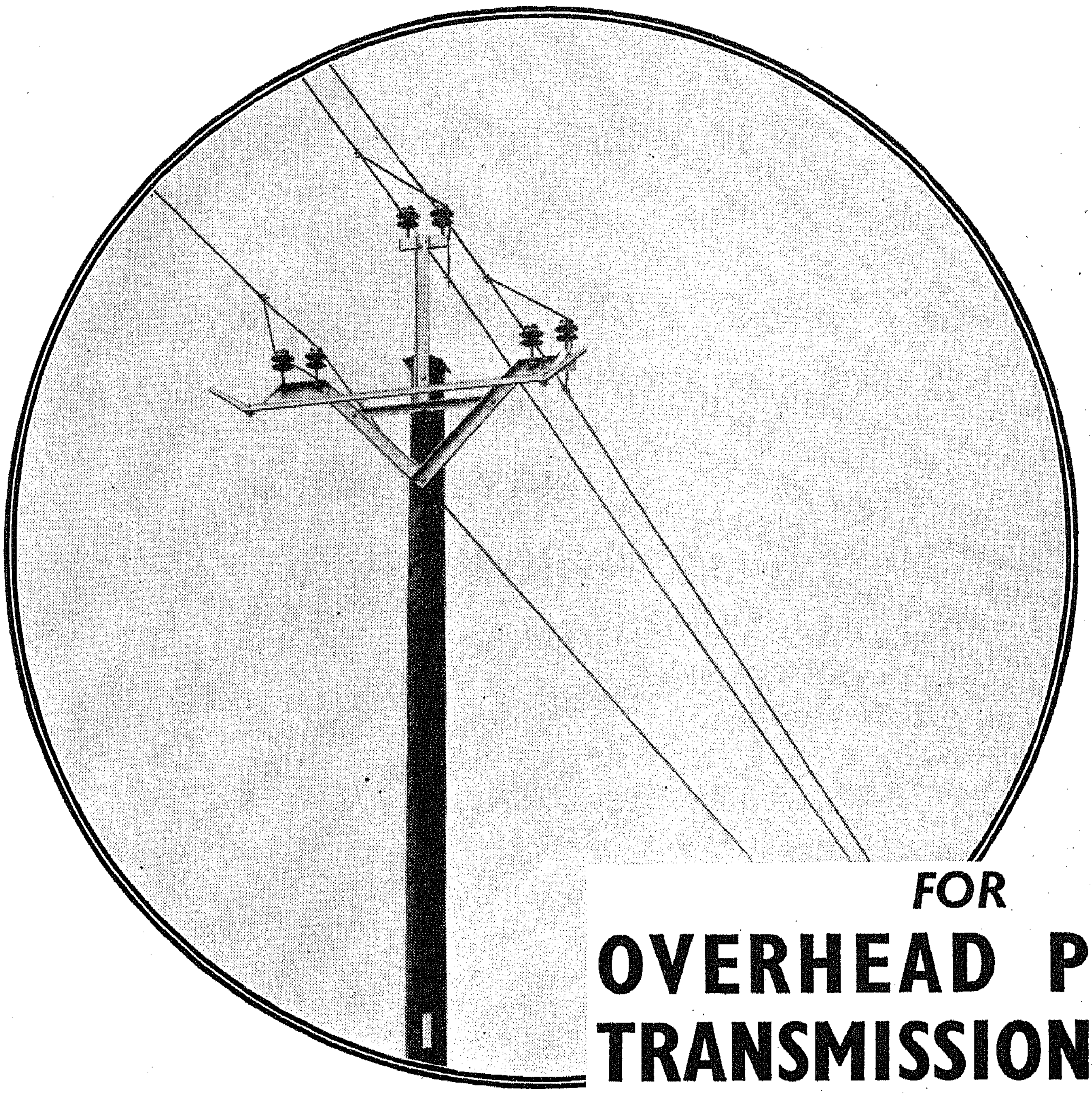

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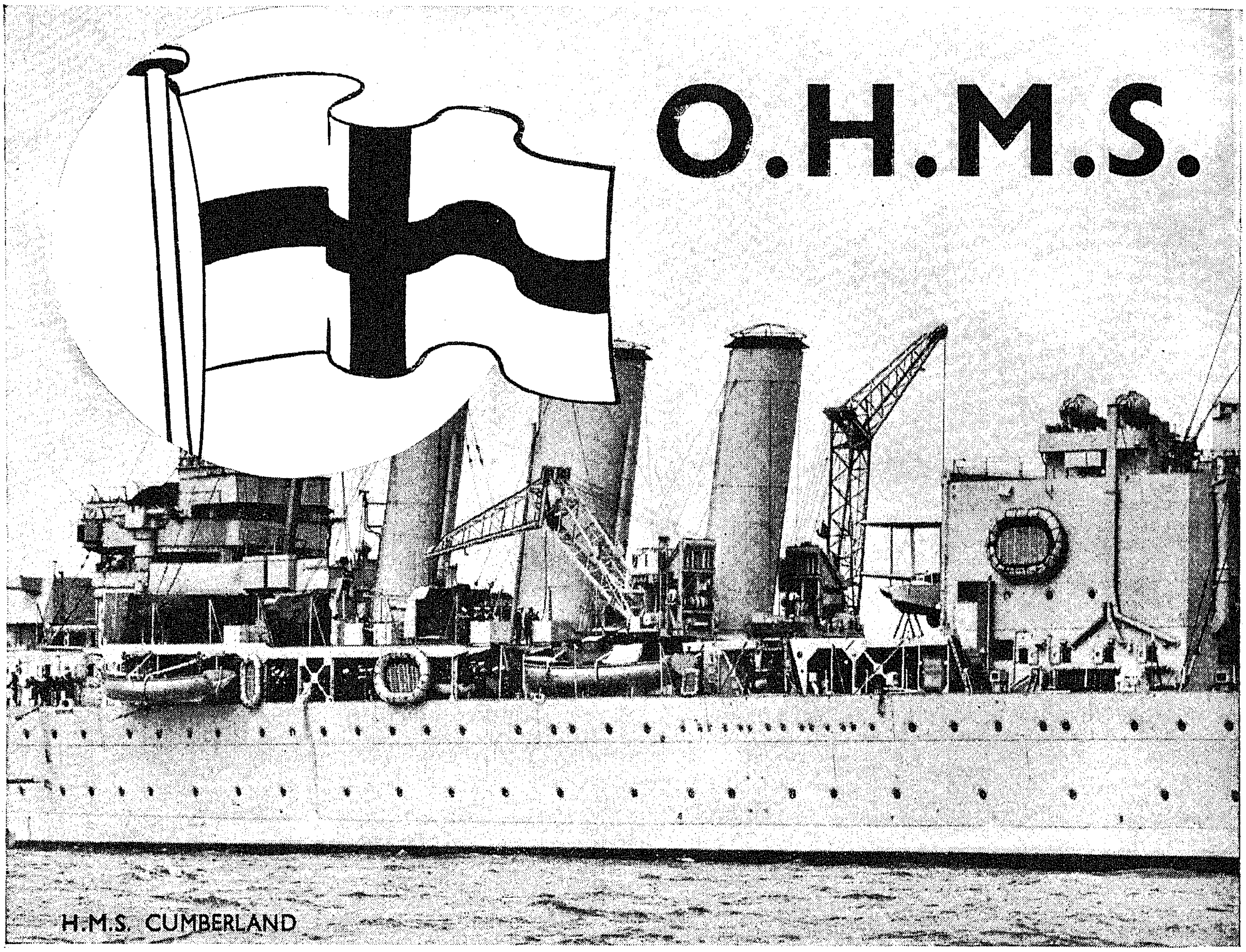

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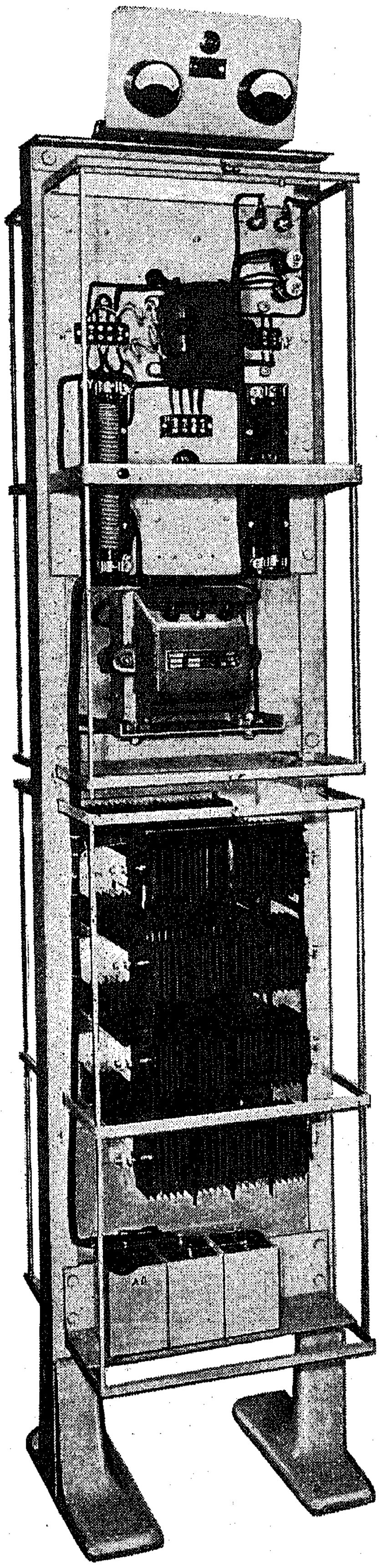

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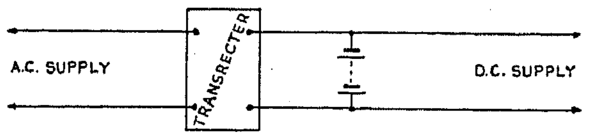

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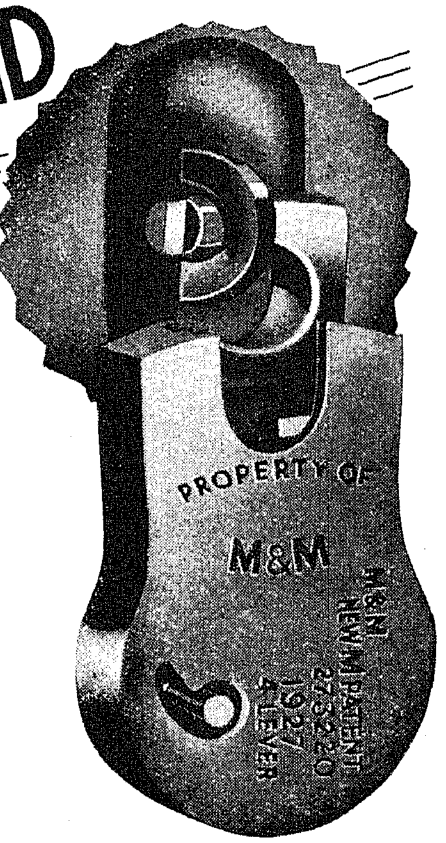

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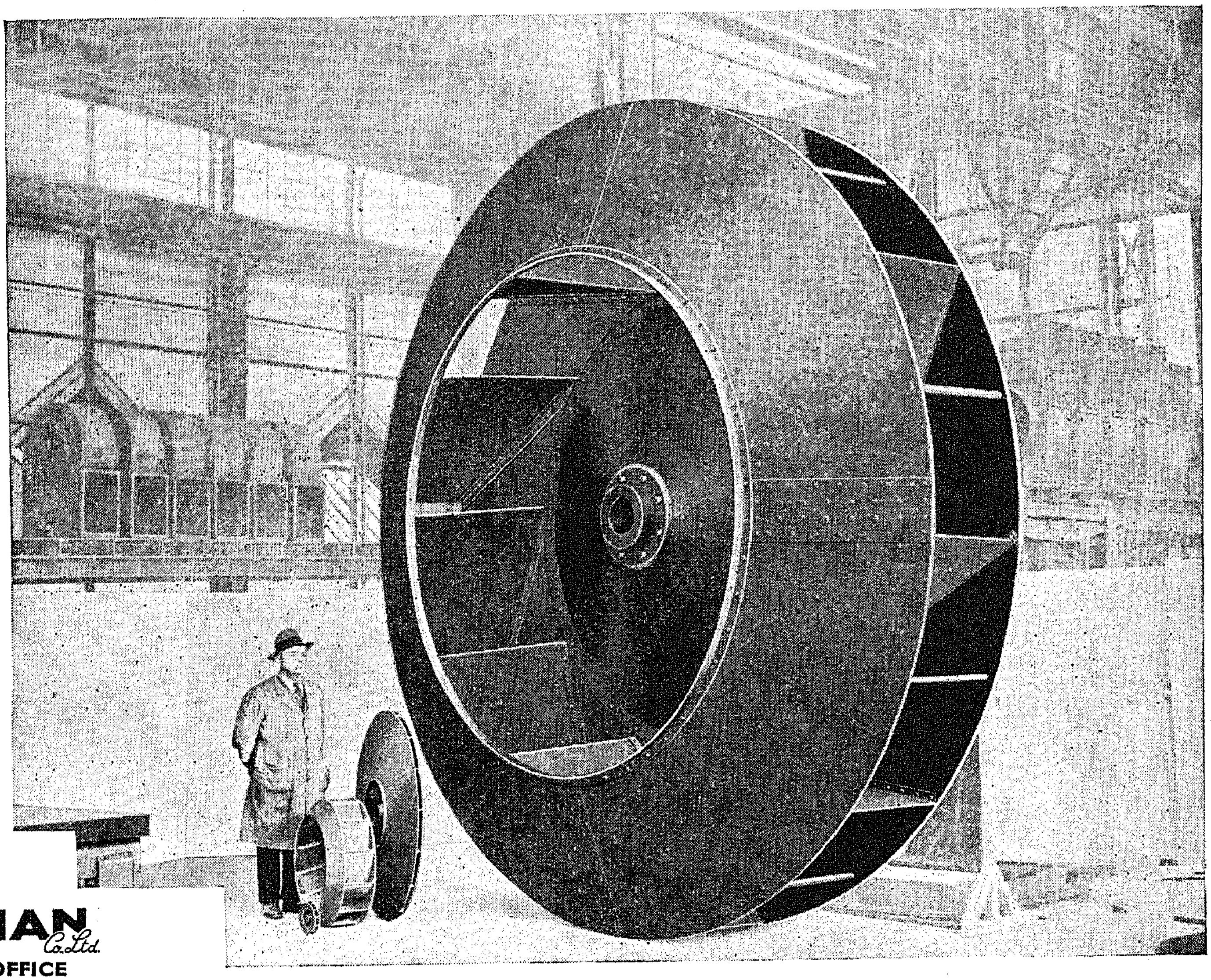

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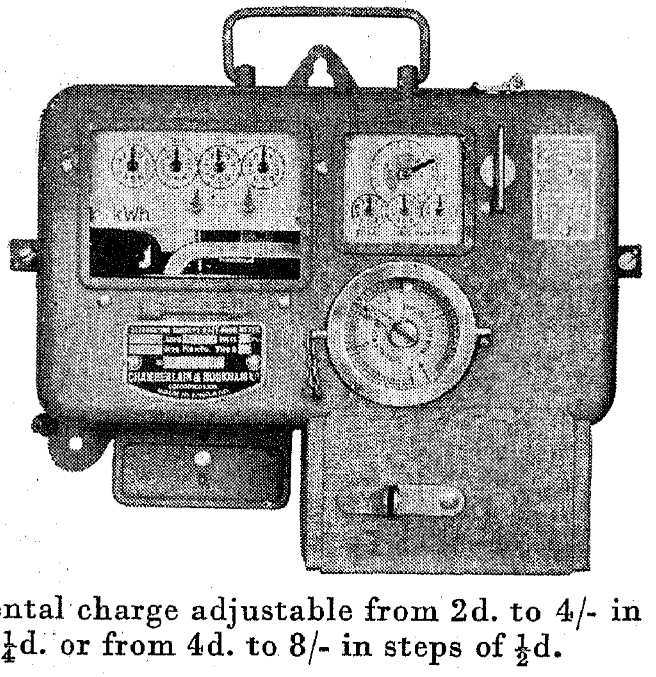

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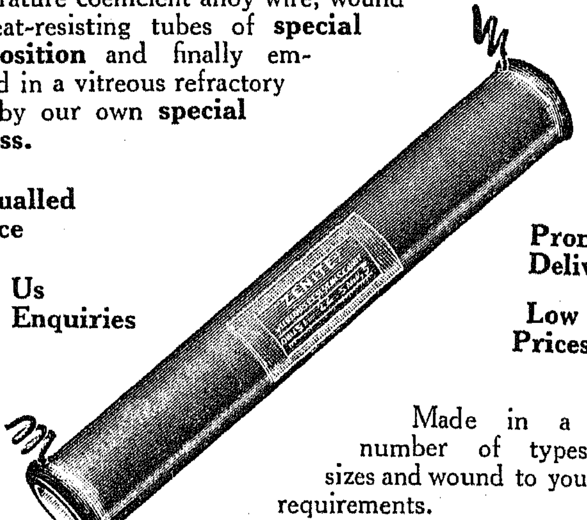

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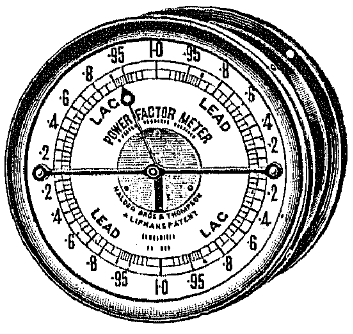

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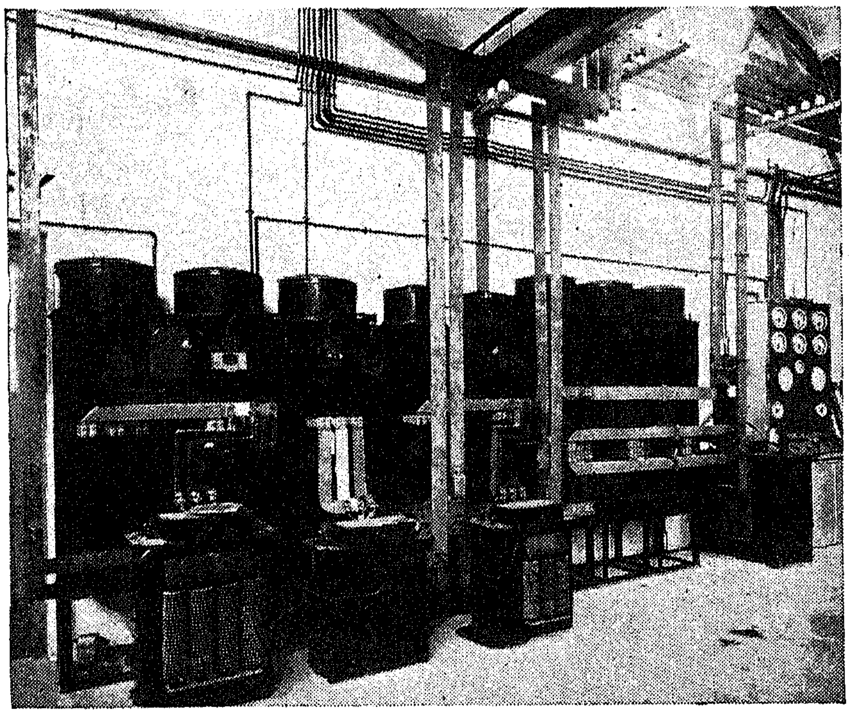

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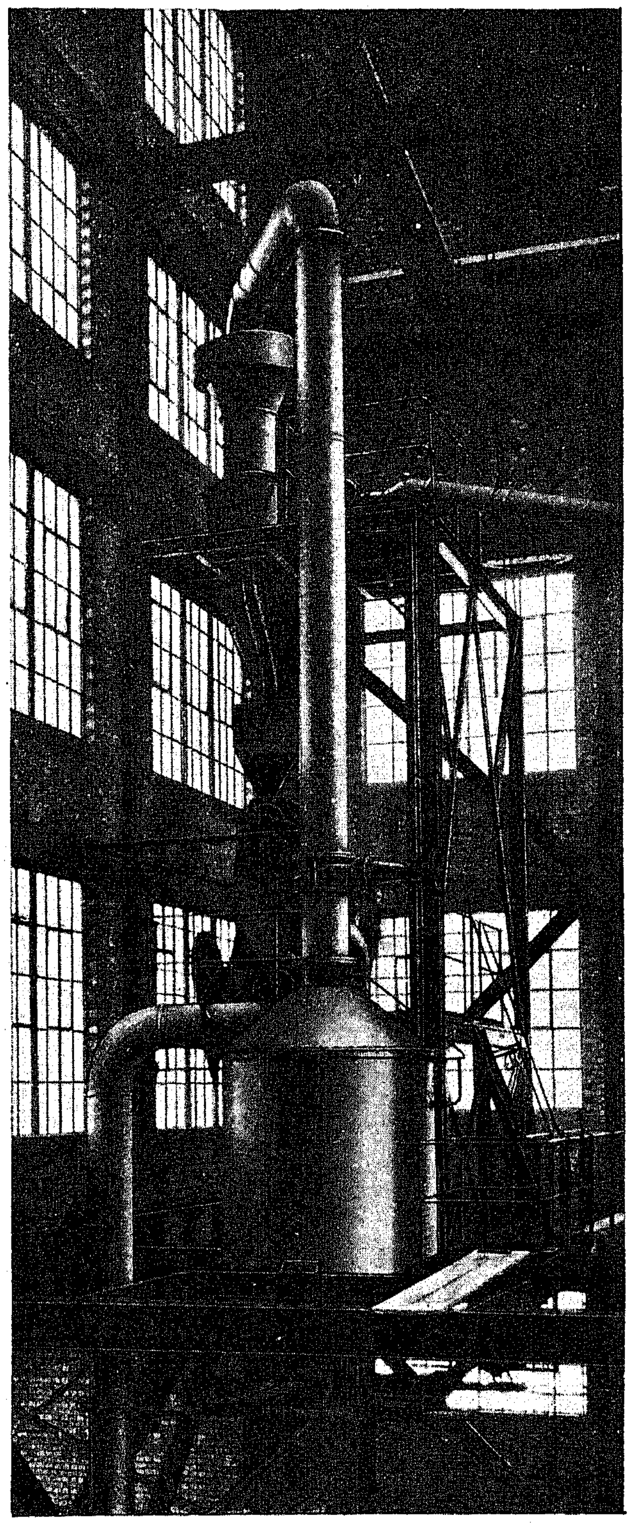

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